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Contents of No. 129.

	PAGE
Solar Surroundings. By R. A. Proctor	257
Effects of the Glacial Period. II. By Robert B. Cook	258
Evolution. By Prof. March, of Yale College	260
The Spread of the Krakatoa Dust Cloud. By A. Cowper Ranyard	261
The Telegraph in a Gale. By W. Slingo	263
How to Select a Life Assurance Office	265
Cycling and Cycles	266
Hymn on the Universe	267
Occultations in a Three-inch Telescope (*Illus.*)	267
The Ornithorhynchus (*Illus.*)	269
River Action on Land	270
Editorial Gossip	272
Correspondence	273
Our Paradox Column	275
Our Mathematical Column	276
Our Whist Column	277
Our Chess Column	278

SOLAR SURROUNDINGS.*

By Richard A. Proctor.

THE LATEST SCIENTIFIC DISCOVERIES ABOUT THE SUN'S CORONA—PHOTOGRAPHS OF IT AT LAST—A GREAT SCIENTIFIC TRUTH SEEMINGLY ESTABLISHED—FUTURE OF THE INQUIRY.

DR. WILLIAM HUGGINS, the eminent physicist and astronomer, entertains the confident belief that he has succeeded in photographing the solar corona without the aid of a total solar eclipse. I am myself not quite convinced that what he has photographed is really the corona, though I would fain hope so. And yet the evidence seems strong enough. His method is simple and probably well known not only to men of science in America, but to most of that large population there which, without being scientific, takes interest in scientific matters. I therefore only sketch it, and that lightly.

He takes advantage of the fact that a large proportion of the light of the corona belongs to the violet end of the spectrum, and uses absorptive media which allow this sort of light and this only to pass freely through. Then, when the photographing telescope is turned toward the sun and the coronal region, the violet light of the corona, which is relatively strong, only has to contend against the violet light from the sky around the sun's place, and has at least a better chance of making its presence known—in other words, a better chance of recording a recognisable picture of the coronal streamers on the photographic plate, on which necessarily the light from the sky is combined with the light from the corona. It seems clear that, if the image is taken first near the centre of the telescopic field and then near a side, any optical effects due to the structure of the telescope itself must be detected and eliminated. Streamers simulating a coronal appearance could not possibly be alike in both positions. So any other purely instrumental peculiarities can, it would seem, be corrected. As for any coronal streamers caused by our own atmosphere, they must be corrected if we take pictures on different days or at different hours. If, under such varying conditions, we find that still certain streamers remain which can be recognised as the same in all the different pictures, it certainly seems as though there must be true coronal streamers. This is what Dr. Huggins claims to have done, and it is what his pictures really seem to show that he has done. So that I scarcely know how to justify the doubts which yet I cannot help entertaining. These streamers are so faint and shadowy (though that of course they could not but be), it is so easy "to make believe a good deal," as Dick Swiveller puts it, in looking at appearances so delicate, especially when (as in my own case) we wish very much to believe that a great scientific triumph has been achieved, that an excess of caution comes over me, and despite the agreement of men so competent to judge as Dr. Huggins, Professor Stokes, and Captain Abney, my mind in this matter "asks for more."

Perhaps one would not be so ready to entertain still a little doubt were it not that the matter is one which can so very readily be tested. Dr. Huggins's method is one which can be applied under especially favourable conditions in the clear skies of America. There are also in America magnificent instruments for testing the method. I should be glad to learn that the mantle of my late most esteemed friend Dr. Henry Draper had fallen on a successor as zealous in the cause of science as he was; nay, even that the instruments he employed so successfully had been directed again to the class of work for which he made them, but with a slight change of subject. Solar photography is making great progress in England; but we have not the favourable conditions which exist in America. It has even been said by a French author, who under the *nom de plume* of Max O'Rell (impossible name!) has recently discussed John Bull and John's Island, that we photograph the sun in England whenever we get the chance lest we should forget him. Without being quite so bad as that, our atmosphere is certainly not the best suited in the world for the very delicate and difficult problem attacked by Dr. Huggins. (Professor Daniel Draper has indeed shown that out of 4,449 possible hours of sunshine, New York had 2,936 actual sunshine hours in 1878, and 3,101 in 1879, say in round numbers 3,000 hours; whereas at Greenwich, with only two hours less of possible sunshine, there were but 1,245 hours of actual sunshine in 1878 and 977 in 1879, an average of 1,111 hours only.)

It may perhaps be thought, by some who have noted the supposed discovery made by Dr. Hastings and Professor Holden during the eclipse of May last, that astronomy ought to assure itself that the corona exists, before attempting to photograph it. If the corona has really been proved to be merely a phenomenon of diffraction, as has been so confidently and also so strangely asserted, the diffraction taking place at the moon's edge, then, of course, when the moon is not there to produce that diffraction corona, it is idle to attempt to photograph what—in that case—has no existence even as an optical phenomenon.

It has been with not a little surprise that the news of this noteworthy discovery has been received by astronomers. An observation which, if it prove anything, proves only what everyone knew must be the case—viz., that light passing close by the moon in total eclipse undergoes diffraction—is astoundingly accepted as explaining the solar corona with its complex structure, its long streamers, its faint extension along the zodiac even beyond the streamers five millions of miles in length seen by Professor Cleveland Abbe in 1878, and by General Myer in 1869. It is perfectly well known that diffraction could account only for a fine coronal ring of light, not even for the inner bright corona, still less for the structured corona

* From the *New York Tribune*.

near the sun, and least of all for the long streamers. Yet the mere circumstance that Dr. Hastings saw what it was practically certain beforehand he would see if he looked for it—viz., evidence of diffraction—is at once taken as full and complete evidence about matters with which it is not in the remotest degree connected.

The theory that the corona is not a solar appendage was not altogether an unreasonable, though it was a demonstrably wrong theory, fifteen years ago. It was clear even then to those who considered the matter attentively that none of the non-solar theories which had up to that time been advanced (including the diffraction theory discussed half a century ago by Baden Powell) were sound. But even the scientific world has been slow to accept the results of mere reasoning; so that in 1869, when the celebrated American eclipse occurred, astronomers were beginning to hope that photography would dispose of the solar corona as it had already disposed of the solar prominences. There had been some who denied that the coloured prominences could belong to the sun, pointing to difficulties akin to those which Mr. Larkin urges, I see, in *The Kansas City Review*, against the doctrine that the corona is solar. Then photography, showing in successive views of the totlaity in 1860 how the moon passes athwart the coloured flames, disposed definitely of the lunar and atmospheric theories of the garnets round a brooch of jet, as the coloured flames had been poetically called. Photography in 1870 did the like for the corona. An American photographer at Xerez, in Spain, and an English photographer at Syracuse, in Sicily, showed in their views the same radiations, rifts, gaps, and general structure in the corona, which could never have happened if the atmospheric glare, the lunar explanation, or any other but the solar theory of the corona had been sound. In 1871, in India, six photographs taken at Baikul, close to the sea-shore and six taken at Ootacamund, some 10,000 feet above the sea-level, showed the same coronal features, all twelve of them. This was not so much a demonstration as *the first easy proof* of the solar nature of the corona,—for even a schoolboy (not Macaulay's schoolboy, who knew everything, but a real one) could see that, unless the corona were far beyond our atmosphere and far also beyond the moon, it could not possibly show the same features as seen, not only from stations hundreds of miles apart, but also at the beginning of totality when the moon's eastern edge is just hiding the corresponding edge of the sun, and throughout totality to the last moment when the western edges of the globes are in apparent contact. Consider the mighty shadow of the moon sweeping along past and over the observer, remembering what the shadow in our air really is, a great cylindrical (really a frustum of a cone but very nearly cylindrical) region of darkness from fifty to a hundred miles or so in diameter; and see the impossibility that when the observer is on the extreme eastern side and on the extreme western side of that shadowed region he should see the same appearance in the air, or anywhere but in a region many millions of miles away, as around the sun. Add the impossibility that at stations two or three hundred miles apart the same appearances are seen, and not only seen but pictured by the unerring pencil of photography.

Yet it must be admitted that a certain interpretation of the corona as a solar appendage is so full of difficulties that one cannot wonder at its having proved a stumbling block to many. I mean the view that the corona is a solar atmosphere. The existence of gaseous matter in the corona does not any more prove, as some seem to imagine, that the corona is an atmospheric envelope, that the existence of gaseous matter in comets, demonstrated over and over again, proves that comets form an atmospheric envelope of the sun. The whole aspect of the corona seems to me to show unmistakably that the several parts of that solar appendage are as free from atmospheric association with the sun as are meteor streams and the heads and tails of comets. I doubt even for my own part whether what we call the visible surface of the sun indicates the extension of a continuous solar atmosphere to that distance from the sun's centre. And that the sierra (which some still call the "chromosphere," a word as correct and pleasing as "photograph" to a classical ear) is not really an atmospheric envelope, in the correct sense of the expression, seems clear when we consider its depth and the inconceivable pressures which would exist at the base of such an atmosphere under the solar gravity, exceeding more than twenty-seven fold that at the earth's surface.

If Dr. Huggins's photographs of the corona are real, the doubts even of those not capable of understanding the photographic evidence already obtained will be dispelled; and to question the solar nature of the corona will be held as obviously absurd as it would now be in the presence of the daily study of the solar prominences to maintain that *they* are only phenomena of diffraction.

Rather strangely in the same number of the *New York Tribune* the following remarks from Professor Young, of Princeton, N.J., appear:—Granting for the moment that the corona is in part and largely composed of an envelope of exceedingly rare gaseous matter around the sun—then we may call it an atmosphere, because being gaseous, and attached to a cosmical body, it bears to that body a relation analogous to that borne by our atmosphere to the earth itself. So far the term is a proper one. But now, further, and on the contrary, the term "atmosphere" carries with it to most persons certain ideas as to the distribution of temperature, density, &c., in its different parts, which are based on the fact that our terrestrial atmosphere is nearly quiescent and in static equilibrium under the force of gravity, with a temperature not more than two or three hundred degrees above the absolute zero, while the density of the portion accessible to human observation is very considerable. On the sun the conditions are immensely, and almost inconceivably, different, so that the term "atmosphere" becomes a very misleading one. There the equilibrium, so far as there is any, is dynamical, not statical, and the density, temperature, and condition of the gaseous substance is far more nearly that of the residual gas in a Crookes's vacuum tube through which an induction coil is sending electrical discharges; so different from that of ordinary air that Crookes thought he had found a fourth state of matter, bearing some such relation to the gaseous state as the gaseous does to the liquid.

EFFECTS OF THE GLACIAL PERIOD.

II.—ORGANIC EFFECTS.

By Robert B. Cook.

WHEN we remember that the climate of the Glacial Period was preceded by an unusually warm and temperate one, evidenced by the remains of a sub-tropical fauna and flora found in the Eocene and Miocene deposits of England and the Continent, it is very obvious that the great cold of the Glacial climate must have produced many and various effects upon the organic beings which then inhabited those parts of the world.

In the first place, many species which were unable to adapt themselves to the changed condition of the climate by migration or otherwise would, undoubtedly, perish and become *extinct;* the absence from our present fauna and flora of a great number of animals and plants which existed here in pre-glacial times being accounted for in this way. Besides, if the supposition alluded to in our last article, that the close of the Glacial Period was accompanied by great floods, due to the melting of the ice, be correct, such floods would account for the extinction of numerous other forms of life which existed in plenty during that period before their occurrence. The total absence at the present day from countries like Siberia of such animals as the mammoth has been explained in this manner. But, however such extinction may have been brought about, it is certain that numerous animals which once roamed the plains of Europe, during the Glacial Period, have now become extinct, and are found no longer in any part of their ancient abodes. Among such animals may be mentioned the mammoth, and other species of elephant, rhinoceros, hippopotamus, bear, lion, hyæna, and many smaller animals now extinct.

More important, however, than any direct or indirect effect of the Glacial Period in the *extermination* of species, are the great changes which that period produced in the *geographical distribution* and *modification* of species.

First, let us endeavour to picture very briefly the *changes in geographical distribution* effected by the Glacial Period. Before the commencement of that period we have evidence that the inhabitants of the northern part of the world were temperate, and in many cases sub-tropical in character, thus indicating a much warmer climate in the northern hemisphere generally. But as during Pliocene times the temperature gradually diminished, these temperate and sub-tropical forms of life would be compelled to migrate southwards, their places being supplied by the more northerly forms which would then invade the temperate regions, until, by the time that the cold had reached its greatest intensity during the Glacial Period, an Arctic fauna and flora would have spread itself over the whole of the north temperate zone. Then, when the amelioration of the climate began, and very slowly and intermittently progressed, would begin and progress in a similar manner the gradual retreat of animals and plants to the north. But such retreat would be by no means such a simple matter as the invasion had comparatively been. Some of the invaders, no doubt, became themselves adapted to the change of climate, and formed settlements in the south, from whence their modified descendants have since spread into adjacent countries; others, in the neighbourhood of mountains, instead of taking part in the general retreat to the north, gained the same object of adaptation to climate by ascending such mountains and settling there. Thus, as we saw in a previous paper, when we considered this same subject as an "evidence" of the Glacial Period, we now have a number of Arctic forms of life, isolated on several mountain summits in the temperate regions, having been left by their retreating brethren in such situations, where they have become surrounded by temperate forms, which now cut them off from their allies who have reached a home in the Arctic regions of the north. The similarity which also characterises some of the so-called "representative species" of Europe and America is also explicable by a common southward migration, caused in both continents by the glacial climate, of forms of life which formerly lived together and spread over the whole of the circumpolar land, which in pre-glacial times enjoyed a comparatively mild climate. The whole subject of the effect of the Glacial Period upon the geographical distribution of species is one which, owing to the complex relationship of one species to another, it is difficult adequately to understand, for we may rest assured that, in addition to the well-ascertained effects that we can see, the Glacial Period also produced numerous other effects in distribution, both direct and indirect, which it is difficult, if not impossible, for us after such a lapse of time to discover. It is certain, however, that the present geographical distribution of animals and plants has been, in a great measure, determined by the geological episode of the Glacial Period.

With regard to the effects of the Glacial Period in the *modification* of species, such subject is also attended, to a considerable degree, by the difficulties just alluded to. There are, however, one or two instances in which such effects are sufficiently obvious, and these will now claim our attention.

When we consider that Arctic animals are generally *white* either all the year round or during the winter season, I think it is by no means improbable that many of them first obtained during the Glacial Period the white colour which they now possess either always or seasonally. For example, the polar bear and hare, living in regions where snow is always on the ground, are always white; while the ptarmigan, ermine, and Arctic fox, which inhabit countries where the snow is melted during summer, possess their white covering only so long as the snow remains. This white colour, which serves in snow-covered lands as a protection against enemies, and enables its possessors to approach their prey unobserved, has doubtless been gained by the agency of natural selection, or the survival and propagation of the white varieties, which by their colour possessed an advantage in attacking or escaping from their enemies, and the extermination of the dark varieties, which by their lack of such advantageous colouring were exposed to early destruction either by starvation from inability to capture their prey, or by the raids of their enemies. Probably, however, the Glacial Period was the cause which set this agency in motion, for before that lengthy period of ice and snow, there would be no need, during the warm, pre-glacial times, for the special white dress of Arctic species.

But perhaps the most interesting effect of the Glacial Period in the modification of species has been the wonderful *instinct of migration in birds* to which it in all probability gave rise. The vast numbers of swallows and other summer visitors to our islands which with the approach of autumn depart to the South in search of a more abundant food supply, and their regular return in spring, furnished for a long time an inexplicable mystery to those who were content with the confession of ignorance involved in the belief that the migratory instinct had been mysteriously implanted in some species and not in others at the time of their creation. But since the birth of the doctrine of development a more satisfactory answer has been found to a problem which it was formerly considered useless, if not impious, to attempt to solve. We have seen that there is evidence from fossil remains that a warm climate, during which evergreens and insects flourished in high northerly latitudes, preceded the Glacial epoch. Such being the case, we may fairly assume that the birds which now migrate in winter then resided permanently in the regions where they now breed, for, so long as there was sufficient warmth and food supply all the year round, there would be no motive for migration. But the gradual decrease of temperature which culminated in the Glacial Period would, in all probability, by first cutting off the food supply during mid-winter, compel the birds to migrate at that time of the year, returning by force of habit to their breeding-places in the spring. Probably at

first such migrations were performed over very short distances; but as the cold gradually increased, and the winters, even in the temperate zone, became more severe, the winterly migrations would of necessity have to be gradually extended further; and as the habit of returning during the summer to the original breeding-ground had no doubt become fixed by inheritance, it would still continue, and so would arise the phenomena of birds breeding during the short summer in the north, and undertaking long migrations in search of food and warmth during the remainder of the year. Both migrations, the return in spring and summer to the original breeding ground, and the winter departure to other climes for food and warmth, continuing through a period of several thousands of years, would become firmly established by inheritance as fixed habits; and so we may explain, not only why the swallows and other birds leave us in autumn —a failure in the supply of their insect food, which compels them to migrate or else perish, combined with the inherited habit, rendering an explanation of their departure comparatively easy—but also the much more difficult question why in spring they leave their warm abodes in the south to visit again for breeding purposes the old northern homes of the respective species to which they belong.

In conclusion, I would repeat that it is almost impossible now to ascertain all the effects which the Glacial Period has produced upon the world in which we live and its inhabitants. Those, however, which we can ascertain are full of the greatest interest, and may afford us unlimited pleasure in our contemplations on Nature, during our holidays among the mountains, the lakes, or in the country.

EVOLUTION.*

By Professor Marsh, of Yale College.

MODERN science and its methods may be said to date back only to the beginning of the present century; and at this time the first scientific theory of organic evotion was advanced by Lamarck. During the twenty centuries before, a few far-seeing men, from Aristotle to Buffon, seem to have had glimpses of the light, but the dense ignorance and superstition which surrounded them soon enveloped it again in darkness.

Before the beginning of the present century, it was impossible for evolution to find a general acceptance, as the amount of scientific knowledge then accumulated was too small to sustain it. Hence, the various writers before Lamarck who had suggested hypotheses of the development had based them upon general reasoning, or upon facts too scanty to withstand the objections naturally urged against new ideas.

With the opening of the nineteenth century, however, the new era in science began. Here, at the very beginning, the names of Cuvier and Lamarck stand forth pre-eminent; and the progress of natural science from that day to the present is largely due to their labours. Cuvier laid the foundation of the study of vertebrate animals, living and extinct, but with all his vast knowledge he was enslaved by the traditions of the past. Although the evidence was before him, pointing directly to evolution, he gave the authority of his great name in favour of the permanence of species.

Lamarck made a special study of invertebrate animals, and his investigations soon led him to the belief that living species were descended from those now extinct. In this conclusion he found the germ of a theory of development, which he advocated earnestly and philosophically, and thus prepared the way for the doctrine of evolution, as we know it to-day.

The methods of scientific investigation introduced by Cuvier and Lamarck had already brought to light a vast array of facts which could not otherwise have been accumulated, and these rendered the establishment of the doctrine of evolution for the first time possible. But the time was not yet ripe. Cuvier opposed the new idea with all his authority. The great contest between him and Geoffroy Saint Hilaire, the strongest advocate of Lamarck's views, is well known. Authority, which in the past had been so powerful in defence of tradition and creed, still held sway, and, through its influence, evolution was pronounced to be without foundation. This triumph of Cuvier delayed the progress of evolution for half a century.

During this period, however, the advance in all departments of science was constant, and the mass of facts brought together was continually suggesting new lines of research, and new solutions of old problems. In geology, the old idea of catastrophes was gradually replaced by that of uniform changes still in progress; but the corollary to this proposition, that life, also, had been continuous on the earth, was as yet only suggested. In the physical world the great law of the correlation of forces had been advanced, and received with favour; but, in the organic world, the miraculous creation of each separate species was firmly believed by the great mass of educated men. The very recent appearance of man on the earth and his creation independent of the rest of the animal kingdom were scarcely questioned at the close of the first half of the present century.

When the second half of the century began, the accumulation of scientific knowledge was sufficient for the foundation of a doctrine of evolution which no authority could suppress and no objections overthrow. The materials on which it was to be based were not preserved alone in the great centres of scientific thought, but a thousand quiet workers in science, many of them in remote localities, had now the facts before them to suggest a solution of that mystery of mysteries, the Origin of Species.

In the first decade of the present half century, Darwin, Wallace, Huxley, and Spencer were all at the same time working at one problem, each in his own way, and their united efforts have firmly established the truth of organic evolution. Spencer did not stop to solve the difficulties of organic evolution, but, with that profound philosophic insight which has made him read and honoured by all intelligent men, he made the grand generalisation that the law of organic progress is the law of all progress. To show how clearly, even in the beginning, he comprehended this great truth, let me recall to you one sentence which he wrote five-and-twenty years ago.

"This law of organic progress is the law of all progress. Whether it be in the development of the earth, in the development of life upon its surface, in the development of society, of government, of manufactures, of commerce, of language, literature, science, art, this same evolution of the simple into the complex, through a process of continuous differentiation, holds throughout."

How completely the truth of this statement has since been established you all know full well.

The evolution of life and of the physical world are now supplemented by the evolution of philosophy, of history, of society, and of all else pertaining to human life, until we may say that evolution is the law of all progress, if not the key to all mysteries. These profounder depart-

* A speech delivered at the Spencer Banquet by Professor O. C. Marsh, the eminent American geologist, acting president of the National Academy of Sciences in America, a year and a-half ago.

ments of evolution I leave to others, for, in the few minutes allotted to me, I cannot attempt to give even an outline of the progress of evolution in biology alone.

If, however, I may venture to answer briefly the question, What of evolution to-day? I can only reply: The battle has been fought and won. A few stragglers on each side may still keep up a scattered fire, but the contest is over, and the victors have moved on to other fields.

As to the origin of species, once thought to be the key to the position, no working naturalist of to-day who sees the great problems of life opening one after another before him will waste time in discussing a question already solved. This question, so long regarded as beyond solution, has been worked out by that greatest of naturalists, whose genius all intelligent men now recognise, and whose recent loss the whole civilised world deplores.

Not only do we know to-day that species are not permanent, but every phase of life bears witness to the same general law of change. Genera, families, and the higher groups of animals and plants are now regarded merely as convenient terms to mark progress, which may be altered by any new discovery.

All existing life on the earth is now believed to be connected directly with that of the distant past, and one problem to-day is to trace out the lines of descent. Here embryology and paleontology work together, and the results already secured are most important. The geneaologies of some of the animals now living have been made out with a degree of certainty that amounts to a demonstration, and others must rapidly follow.

In this, and in all other departments of natural science, the doctrine of evolution has brought light out of darkness, and marks out the path of future progress. What the law of gravitation is to astronomy the law of evolution is now to natural science. Evolution is no longer a theory, but a demonstrated truth, accepted by naturalists throughout the world.

The most encouraging feature in natural science—indeed, in all science to-day—is the spirit in which the work is carried on. No authority is recognised which forbids the investigation of any question, however profound; and, with that confidence which success justly brings, no question within the domain of science is now believed to be insoluble; not even the grand problems now before us—the antiquity of the human race, the origin of man, or even the origin of life itself.

THE SPREAD OF THE KRAKATOA DUST-CLOUD.

By A. Cowper Ranyard.

(*Continued from page* 178.)

THE great outbreak of Krakatoa commenced on the afternoon of Sunday, the 26th of August, 1883, and continued with extraordinary violence till about mid-day on Monday, the 27th. There had been a minor outbreak three months previously, on the 20th of May, and the mountain continued smoking and throwing out pumice in small quantities till the great outbreak of Sunday, the 26th of August. On that evening and the following morning an immense amount of material was thrown into the air, so that on the 27th in the neighbourhood of Anger it was as black as night at noon. Capt. W. J. Watson, whose ship was at the time 30 miles from Krakatoa, says, "At noon the darkness was so intense that we had to grope our way about the decks, and although speaking to each other on the poop, we could not see each other."

A great deal of pumice and mud fell in the Sunda Straits and over parts of the islands of Java and Sumatra, and, for a distance of three or four hundred miles round the volcano, a whitish dust was deposited on the decks of ships. On the island of Java it covered the trees and soil so thickly that the Dutch settlers were reminded of the winter appearance of the landscape in Holland.

The cloud of smoke and dust in the upper air seems to have extended with the greatest rapidity towards the west. By the evening of Tuesday, Aug. 28, it had travelled to the Mauritius, a distance of over 3,000 miles. Mr. Meldrum writes in the "Mauritius Meteorological Society's Proceedings" * for Oct. 27, 1883, p. 10: "Shortly after sunset on Aug. 28, the sky and clouds were coloured yellow and red up to the zenith, and before sunrise, on the 29th, the whole of the eastern sky had a fiery-looking appearance, as if there were an extensive conflagration below the horizon. On the three following days the colorations were still more intense."

About the same time an abnormal appearance of the sun was observed at the Seychelles, over a thousand miles to the north of the Mauritius, and three thousand miles due west from Krakatoa. "At the Seychelles, on the morning of the 29th, the sun was more like a full moon than anything, and at sunset on the 28th it looked as it does through a fog or on a frosty day in England."† It will be noticed that at the Seychelles only the dimming of the sun's light was observed; the red colour of the sunsets was not noticed, nor is there any mention of the blue or green appearance of the sun's disc, either here, or at the Mauritius. Probably the particles floating in the air on the 28th and 29th were too large to give rise to the blue colour of the sun which was seen at a greater distance from Krakatoa, and after an interval of time in the neighbourhood of the eruption.

It seems that the haze was much more dense at the Seychelles than at the Mauritius, which may be accounted for by assuming that the Seychelles were near to the centre of a broad stream of dust-laden air, which flowed away from the Sunda Straits, while the island of Mauritius lay on the southern edge of the stream.

We next hear of an unusual appearance of the sun having been observed on the morning of Saturday, August 29, at Cape Coast Castle, more than seven thousand miles away from Krakatoa, nearly in the same direction as the Seychelles. Mr. W. B. Griffith, writing to the *Times* of Dec. 5, gives the following extract from the *Gold Coast Times* of Sept. 14:—

"On the 1st and 2nd of this month the sun was described as being blue in the morning. It seems it rose as usual, and that the clouds which passed over it, from their greater rarity or density, gave it different apparent shades of rose colour, pink, and so on. After the passage of the clouds its appearance through the haze was white, like the moon. In fact, an Englishman is said to have taken it for the moon."

By the morning of Sunday, September 2, similar phenomena were observed twelve thousand miles away from Krakatoa, at Barinas, Venezuela, in South America. Mr. G. J. Symons, in his letter to the *Times* of December 1 (which was the first communication that suggested that dust from the Krakatoa eruption was the cause of the extraordinary sunsets seen in England), states that at Barinas from sunrise to noon the sun looked like burnished

* My thanks are due to Mr. A. Ramsay, secretary of the Royal Society's Krakatoa Committee, for this reference.

† This quotation is taken from Mr. Lockyer's article in the *Times*, of Dec. 8. He does not give the authority from which he quotes.

silver, and from noon to 3 p.m. it was bluish-green. Similar phenomena were also observed on the Sunday afternoon in the island of Trinidad. In a letter from Dr. Arnold to the *Times*, he states that about five o'clock the sun looked like a blue globe, and after dark the bright redness of the heavens gave rise to the idea that there was a fire in the little town of Port of Spain.

It will be noticed that Barinas and Trinidad do not lie very far from the line drawn from Krakatoa through the Seychelles, and produced onward past Cape Coast Castle. This line does not run due west from Krakatoa, but is inclined somewhat northward. Krakatoa is in latitude 6° 9′ south, and Port of Spain is 10° 40′ to the north of the equator. Possibly the central line of the stream was somewhat less inclined to the equator. The silvery appearance of the sun seen at Barinas, but not seen from Trinidad, seems to show that Trinidad was on the northern edge of the stream, and that the dust-cloud was thicker at the more southern station.

If it is assumed that the phenomena observed at Barinas and Trinidad were caused by dust thrown into the air on the previous Sunday in the Sunda Straits, it will follow that matter was carried half round the earth with a velocity of 1,700 miles a day, or 71 miles an hour, kept up for a whole week. Such a supposition at first appeared incredible, especially as there seemed to be evidence to show that the prevailing winds were in the opposite direction.

The barographs or automatic traces made by barometers at the chief meteorological observatories on Aug. 27 and the next five days show a series of well-defined disturbances, the first of which is a rise, followed by a subsequent depression, which, at the European and American stations, corresponds to a difference of more than a tenth of an inch of mercury. At some observatories, as many as seven of such waves, gradually decreasing in magnitude, can be detected, while at other observatories, where the instruments were not so sensitive, only five can be traced. General Strachey has, in a paper communicated to the Royal Society, collected the observations made at all the chief meteorological observatories, and has pretty conclusively shown that a great atmospheric wave started from the region of Krakatoa at about 9 h. 30 m. a.m., on Aug. 27. It spread outwards in all directions, and travelled round the globe, the parts of the wave which had travelled in opposite directions passing through one another somewhere in the antipodes of Java. The wave then returned upon itself to the region of the explosion, and again passed round the globe. The seven irregularities which are recognisable in the barograph curves of several observatories show that the wave must have passed three and a quarter times round the earth before it was sufficiently reduced to make it too weak to affect the instruments.

General Strachey has shown that the velocity of the waves in miles was, for those which travelled from east to west, 674 miles per hour, and for those passing from west to east, 706 miles per hour. The velocity of sound in air, at a temperature of 60° Fahr., is 771·1 miles an hour; with a temperature as low as zero Fahr., the velocity will only be reduced to 724 miles an hour, which is still considerably in excess of the velocities observed. This would seem to indicate that the waves travelled at a great height in the atmosphere where the temperature was considerably below zero. The excess of the velocity of the waves which travelled in the same direction as the earth's motion of revolution, that is from west to east, over that of those which passed in the opposite direction, is about thirty-two miles an hour, which, General Strachey suggests, may be accounted for "by the circumstance that the winds along the paths of the waves would, on the whole, be from the west, which would cause an increase in the velocity of the one set and a diminution in that of the other, so that the observed difference of thirty-two miles would correspond to an average westerly wind of sixteen miles an hour," which he thinks is not improbable.

The path of the wave which passed through Toronto approached very near to the north and south poles, and the velocity in both directions is somewhat less than in the waves which passed over central Europe. The wave which passed northwards over Asia travelled, according to General Strachey, at the rate of 660 miles an hour, or about 15 miles an hour slower than the wave which passed over Great Britain from east to west. This reduction of rate, it is suggested, may be accounted for by the slower velocity of the waves in the cold air of the polar regions.*

* Six hundred and sixty miles an hour is equivalent to 968 feet per second, which corresponds to the velocity of sound in air at a temperature of −76°·5 Fahr. The mean of the velocities of the waves which passed across Europe in an easterly and westerly direction was 690 miles an hour, which corresponds to the velocity of sound in air at about −42° Fahr. The difference of pressure at a great altitude would make no difference in the velocity of the sound-waves. The absence of aqueous vapour might make a small difference, but the velocities of sound, determined experimentally by Capt. Parry during his third voyage to the Arctic Regions at temperatures varying from −38°·5 to +35° Fahr., agree so nearly with the theoretical values derived from the formula I have used in calculating the above-mentioned velocities that it seems probable that the difference caused by the absence of vapour in cold air may be neglected. Theoretically, loud sounds, where the displacement of air is large, should travel a little faster than sounds that are not so loud. The constants in the formula used have been derived from experiments made by observing the flash of a cannon fired at a distance—no doubt the displacement of the air caused by the Krakatoa explosion was very large compared with the displacement caused by the sound-wave from the largest cannon. If such great displacements travel much more quickly than the sound-waves from cannon, we should be forced to assume that the air in which the waves from Krakatoa travelled was colder than the above mentioned temperatures. Capt. Parry and Mr. Fisher, who observed the velocity of sound together, during Parry's second voyage, make a curious remark with regard to their experiments, which bears on this subject. The stations were so near that the human voice could be heard, and they mention that the officer's word of command, "fire," was heard about one beat of the chronometer (½ of a second of time) *after* the report of the gun. Sir G. B. Airy remarks in his treatise on Sound, that he cannot imagine that the acceleration could, at the distance at which Parry observed the cannon, amount to a space of 200 feet. He thinks it more probable that the phenomenon was physiological, and adds: "When the report of a gun or any other violent and sudden noise is heard, it is preceded by the perception of a shock through the bodily frame, the interval in time being a large fraction of a second. From the voice there would be no sensible shock; but the shock from the cannon-explosion might be sensible, and might precede the auditory perception of the report by a time sufficiently long to present itself to the observer's mind before the auditory perception of the voice." But if this were the case, distance from the cannon ought not to have made any difference, and the officer giving the order, as well as the man firing the cannon, ought to have heard the report of the cannon ⅔ of a second before the officer's voice was heard. Judging from my own experience, I do not think that this can be the true explanation. Such experiments might now be repeated with much greater precision. A microphone might be used to register automatically on a chronograph the arrival of a sound made by a clock a second before an explosion of dynamite on a distant mountain, and the time of the flash, and the arrival of the great sound waves could also be registered automatically. The *first* and *second* of the series of the great air-waves from Krakatoa are, in almost all the Barographs, well defined and generally similar in form, commencing with a distinct rise, followed by a fall, the fall being shorter than the rise. These features are followed by a less definite rise, succeeded by a shallow fall, after which there is again a rise, which gradually passes into the more regular trace. The *third* and *fourth* of the disturbances can be traced in all the curves, but they no longer exhibit the same characters, and are usually nothing more than a sudden, sharply-defined rise, though in front of some of these there is a more or less distinct trace of a hollow. The *fifth*

Mr. G. J. Symons, in his interesting letter to the *Times*, of Dec. 1, endeavoured to account for the rapid spread of the cloud of ejected matter in a westerly direction by suggesting that the dust would be left behind by the rotating earth when thrown into the upper regions of the atmosphere, but if matter was thrown to a height of seventy miles above the earth's surface (the height at which the majority of meteors become visible), and was not carried forwards by the air at the higher level, it would only fall behind about 440 miles in the twenty-four hours, whereas we have to account for a westerly velocity of 1,700 miles a day. Mr. Symons has since suggested that the matter might have been carried altogether outside the earth's atmosphere, and so have got a whole hemisphere behind. But this theory is seen to be untenable when we remember that meteoric particles, with an initial velocity of many miles per second, do not, unless they are exceptionally large, get through the outer half of the earth's atmosphere without being driven into vapour. If the height of projection were the cause of the westerly drift, the ashes and large particles would have been left further behind than the fine dust, for the larger particles would experience less resistance, and consequent loss of velocity in passing outwards through the earth's atmosphere than the dust, the larger particles would consequently have been projected upwards to a greater altitude, and have fallen further behind than the dust; but, in fact, we find the larger debris falling near the volcano, and the dust carried to great distances.

It is evident, therefore, that the finer particles must have been carried upwards by heated air, and afterwards have been borne along by the wind. Krakatoa is situated in latitude 6° 9′ S. On Aug. 26, the sun had a northern declination of 10° 20′, so that in its daily course it passed vertically over the region 10° north of the equator, heating it more intensely than the zones to the north or south. The phenomena of the trade winds indicate that there is an ascending current in the region where the sun is vertical, and that the air which rises is chiefly drawn from regions to the north and south, where the velocity of rotation of the earth's surface is less than the velocity of rotation where the heated air rises. We should consequently expect to find the ascending current of heated air lagging behind the surface of the earth beneath it, and giving rise to a strong westerly wind in the upper regions of the atmosphere where the sun is vertical at noon.

The heated air having been drawn from the north and south on the earth's surface flows out again towards the poles in the upper regions, and in time must arrive at a latitude where its rotational velocity is equal to the rotational velocity of the earth's surface beneath it. Beyond this region the current of upper air would appear to turn backwards and flow in a contrary direction relative to the earth's surface—that is, from the west towards the east. If at the time of the eruption the equatorial current had a westerly velocity of 1,700 miles a day, its motion of rotation or velocity round the earth's axis would correspond with the velocity of the earth's surface in latitudes 22° north and south, and further north and south the upper current would be blowing backwards, or in an easterly direction.

It is therefore evident that waves starting from Krakatoa and travelling in the upper air towards the north-west so as to pass across Europe and America would in their whole course round the earth from east to west encounter more adverse winds than waves passing in an opposite direction round the earth from west to east, while in passing round the earth in the equatorial regions the waves would travel 3,400 miles a day more rapidly from east to west than from west to east. What at first appeared contradictory is now seen to be a necessary result of the rising of heated air in the neighbourhood of the equator.

The direction "slightly to north of west" in which the cloud of dust from Krakatoa was carried is also explained, since the sun was vertical in latitude 10° north at the time of the eruption, and the heated air would be rising fastest to the north of Krakatoa, the smoke and lighter dust borne away by the wind would be carried into the main stream flowing backwards or in a westerly direction with greatest velocity in latitude rather more than 10° N., and it would then gradually spread outwards towards the north and south.

We find that the blue and green appearance of the sun and red sunsets were first visible in Ceylon and India on Sept. 9, which is about the time that the great stream of dust and smoke which stretched out to the westward from Krakatoa would have occupied if it had passed entirely round the world and had travelled at the rate of 1,700 miles a day. But the news received from Japan renders it probable that the dust-cloud spread more directly towards the north-east. An extract from the *Japan Gazette* for Sept. 21, 1883, published in *Nature* for April 3, 1884, describes phenomena observed at Tokio on August 30, 31, and Sept. 1, similar to the phenomena observed at Cape Coast Castle on 29th August, 7,000 miles westward from Krakatoa. Tokio is about 2,500 miles from Krakatoa, and the spread of the dust-laden air must consequently have been at the rate of about 600 miles a day towards the north-east.

From the *Chilian Times* of Oct. 27, 1883, I find that the red sunset phenomena were first observed at Carrizai Bajo, about 300 miles north of Valparaiso, about Oct. 5. The sunset phenomena did not attract general attention in England till Nov. 9, 1883.

and *sixth* of the series become less distinct, and are lost at several stations, being usually rises; while the *seventh* faint disturbance is a shallow hollow that can be traced on a few of the curves.

The late Mr. Earnshaw, in an important paper published in the *Philosophical Transactions* for 1860, investigated the velocity of transmission of different parts of a sound wave, and has shown that in the case of a violent sound, generated with extreme force, every part of the wave, with the exception of its rear, is perpetually gaining on the front, and the result is a *constant change of type*, the more condensed parts hurrying towards the front, with velocities greater as their densities are greater. Mr. Earnshaw remarks that "this cannot go on perpetually without its happening at length that a *bore* is formed"; that is, the wave will break forward somewhat as waves break when advancing on a gradually shallowing shore, or up a narrowing tidal river. Mr. Earnshaw also shows that a negative or rarefied bore would be formed in the rear of a wave of rarefaction. The great air-waves from Krakatoa form an excellent illustration of the results theoretically arrived at by Mr. Earnshaw. The vast scale of these waves may be judged of from the fact that the first main-wave or rise took about an hour to pass over the meteorological stations; that is, it was about 700 miles in length. The whole length of the first series of disturbances was between 3,500 miles and 4,000 miles. The series occupies between five and six hours on the time-scales of the Barographs. If the time of the breaking up of the wave and the first formation of the bore could be determined, we could, knowing the length of the wave, determine the acceleration of its central parts, and could form a more accurate estimate of the mean temperature of the air in which the wave was transmitted.

THE TELEGRAPH IN A GALE.

By W. Slingo.

IT has been truly said that the present age may be described as that of electricity, for of a surety in no other branch of science have such rapid and gigantic strides been made as those which have resulted from the scrutiny of many great minds into the whence, the wherefore, and the

whither of electricity. Apart from these considerations, the very subtlety of the force, its incomprehensibility, and the mystery that surrounds it at all times and under all conditions, may well be calculated to imbue one with a peculiar sensation of wonder and amazement. "How wonderful!" is the expression of every visitor to a large and busy telegraph office. The miles of wires about the room, the multitude of mysterious instruments, the thousands of brass "terminals," each of which is known to the engineer by name and number, alike generate feelings of astonishment. It is at once apparent that the telegraphist's operations have no counterpart in the whole domain of social enterprise. And yet all the work that is seen to be going on, the multitude of appointments that are being telegraphically made or broken, the millions of pounds' worth of business that are being similarly transacted, are all more or less at the caprice of the wind and weather. To my mind there is no occupation so interesting, and few things so exciting or so calculated to upset one's mental balance, as to be in charge of a large system of busy telegraph wires during a gale. As I write my mind goes back to the afternoon of Saturday, Jan. 26, 1884, when one of the most violent storms which we have experienced for some years passed over the British Isles. The day had been a quiet one, and those on duty in the central office were composing themselves for the evening. It was known, however, that dirty weather had shown itself in the West; but the effect on the wires had been small, and little danger was feared. At four o'clock in the afternoon all their calculations were upset, and one of the busiest evenings ever known set in. What the effect would have been had the day been any other than on Saturday is inconceivable. However, the clock had struck four, and information was received that one of the nine Glasgow wires was stopped. No sooner had steps been taken to ascertain the locality of the stoppage than another and another gave out, and in a few minutes it became known that the whole of the Trunk line on the Carlisle and Glasgow road, which carried seven wires to Glasgow, was down. Simultaneously the five wires on the Caledonian Railway, between Carlisle and Beattock, gave way, leaving London without any communication to Scotland by the western routes, and stopping also the Belfast wires (going *vid* Carlisle and Stranraer). Such a catalogue was in itself sufficient evidence, were there no other, to indicate the existence of a terrific gale. But it speedily showed its existence over the whole length of the land, wires running north and south falling not in ones and twos, but in batches. By six o'clock the storm had travelled from the Bristol Channel down the Thames valley to London; the gale increased in strength as time went on, and reached its height in London at about 7.30 p.m. But those locked up in the office, while having evidence of the devastation that was going on around, heard and saw nothing of the storm itself. Too busy to look out of window, they did not see the rain, nor did they hear the roaring of the wind. While all this had been going on, while wires were being tested north and south, the centre of the gale which wrecked the West coast wires travelled across the country, and eventually reached the East coast. Running through Newcastle were nine wires to Glasgow, Edinburgh, Dundee, and Aberdeen, and these shared the fate of their brethren on the other side of the Pennine chain. Down they came, wires, insulators, and poles; some pulled out of the ground, some snapped off short at the ground, some broken in the middle, and then hurled by the fury of the gale into the road, or more generally into adjoining fields. When this feat had been accomplished the word was passed, "Stopped to Scotland and the North of Ireland." In all there were, of the wires running out of London, upwards of 130 known to be broken down. As the wind has no respect for persons, "cross-country" wires (or wires connecting two provincial towns) suffered equally with the London wires, so that, for example, Scotland had no communication whatever with the south. Space will not permit a detailed review of all the stoppages, but a few comments on the general features of the breakdown may not be out of place. One remarkable point worthy of observation is that the general nature of the stoppages, and the routes principally affected indicate pretty clearly the direction and limits of the gale, its approximate force, and the line of greatest energy. From past experience it is easy to conceive that a gale has some one central line or belt, along which it travels, and in the path of which the greatest damage will be wrought. There were presented, however, in this belt or zone features of a most remarkable nature. At and previous to the commencement of the storm sleet fell somewhat heavily, but it soon gave place to snow. This, on coming into contact with the wires, froze on to them, and formed, as it were, heavy ropes of ice two to four inches in diameter. Great assistance was thus rendered to the gale in its destructive efforts. Two or three days afterwards long tubes of ice were found on the ground under the wires, each tube having a longitudinal slit which had been made by the wire in thawing—if I may use such a word—itself out. It is a noteworthy fact that in the whole history of telegraphy the only occasions on which these features were exhibited were in 1866, when the central belt, accompanied by snow and frost, passed south of London; in 1876, when it traversed the Midlands; and lastly, in 1884, when, as shown above, it passed along the Border. The belt in this particular storm was about 25 miles broad, and passed from west to east, being bounded on the north side by Ayr, Hawick, and Alnwick, and on the south side by Girvan, Dumfries, and Morpeth. Residents in the South of England have a vivid recollection of the violence of the storm, but strong as it was it is a remarkable fact that comparatively very few of the wires travelling east and west were affected. This fact, coupled with the circumstance that wires running at right angles to these were almost invariably impaired, afforded a sure indication to those on the spot of the direction of the storm, and the practical annihilation of all the three Scotch routes confirmed the inference, besides testifying to its greater virulence in the north. The comparative immunity subsequently shown to have been enjoyed by the wires north of Ayr helped to define the limits of the gale's central zone. To that zone a brief consideration will now be given. Among the laws of telegraph construction is one that requires the poles, where it is practicable, to be fixed with reference to the prevailing winds, on the leeward side of the road, the object being that in the event of a storm breaking the wires or poles they shall fall into adjacent fields instead of blocking the road. Trees falling from the opposite or windward side of the road would also be less likely to reach the wires and drag them down. Falling trees are a prolific source of injury to telegraph wires, and no amount of care and forethought is sufficient to guard against them. Sometimes their large surface cause them to be carried a considerable distance. In one instance, at Edgerston, on the Newcastle and Jedburgh road, a large tree fell, and "knocked one double pole* and all its fittings to pieces." In another portion of the road the large trees sheltered the wires; but, at Langlee, a tree 9 ft. in girth, broke off "about 6 ft. from the ground

* A double pole is in reality two poles fixed side by side, braced together above and below the ground, and inclining towards each other.

and turned a summersault over the line, only breaking one wire." While, however, trees did a deal of damage, the greater part of the injury sustained resulted from the pressure of the wind on the wires. Near Capheaton, "for about thirty-five to forty pole-lengths, the line was wrecked, fifteen poles being broken off short and many wires, insulators, and arms broken. Struts* were largely used, and proved to be quite useless." At Kirkwhelpington, where even worse damage was wrought, "seven double poles had been blown out of the earth, one actually on the other side of the road having drawn its stay,† and snapped its cross-brace in the middle." Near Horsley, "nine double poles were actually smashed to pieces, breaking at various points between the ground-line and 6 ft. above." So goes the record throughout the breadth of the zone, the number of breakages only varying, sometimes one or two poles, sometimes twenty-five became *hors de combat*. In one curve the seventeen wires were pulling against the wind with a united breaking strain of at least 22,000 lb., but all to no purpose.

The work involved in repairing such a wreckage is inconceivable, but the wires were got through in batches until, on Tuesday, the 12th of February, all the East-coast wires were "in working order." Had there been room, a few comments on the phases of the Western breakdown would have found a place. Suffice it however, to say that the breakdown on the Carlisle and Glasgow road had much in common with what was noticed on the Newcastle and Jedburgh road, while on the Caledonian Railway the wires were torn down for miles by passing trains. A fortnight or three weeks were spent in making reparations, but in the mean time the London staff was often at its wits' end in the effort to establish communication with Scotland. Usually there are 19 wires, all of which were stopped on Saturday evening. The first line was made up to Edinburgh on Monday afternoon. This channel of communication was soon closed, and another wire was got through to Edinburgh on the following evening. On Wednesday afternoon five or six wires were made up, some of them by utilising the good portions of several bad wires. Eventually the normal condition of things was restored, but many a year will go by ere the gale of Jan. 26, 1884, will be forgotten by telegraphists.

HOW TO SELECT A LIFE ASSURANCE OFFICE.

FROM the office having a long list of directors of well-known names, or with high-sounding titles? From the glowing statements in a report or prospectus? From the "advice gratis" of some kind friend or persuasive agent? From the offer of an exceptionally low premium? From the fact of large bonuses having been distributed? No! by none of these "royal roads"—that is, if you wish to make your investment to the best advantage, and to have no half doubts and fears after the deed is done. No, but from the facts given in the yearly returns to Parliament, collectedly published in a Blue Book, at the price of a few shillings, and a patient investigation thereby of the actual financial position of the office, and from other inquiries as to the status of the office. Certainly not very inviting work this—this digesting of figures and making of calculations—but for those who are pertinacious enough to do it, and know the value of the maxim that "Blessed are the hands that help themselves," I will point out what are the facts their inquiries should aim at ascertaining.

First—and this is the question of paramount importance—what security is offered, what ability has the office of meeting its death claims? Now, those that read my former article will remember that on principle an office receives what it has to pay back, and the premium it receives from an assured is larger or smaller according as the death claim will the sooner or later presumably have to be met. Hence the office ought to have a fund in hand—a life-fund proportionate to the premiums they are receiving, and the inquiry on this head may most conveniently be answered by ascertaining the amount of the life fund of the office for every £1 of premiums received. This can easily be done by dividing the amount of the life fund by the amount of premiums. Say, for instance, the premiums are £242,618, and the life fund is £4,337,492, the answer would be £17. 17s. 7d.

Secondly, how is the office managed? Are the assets well invested and easily realisable? In other words, taking the item "Interest and dividends," what rate of interest does it show the assets to be returning? What, too, is the amount of outstanding interest? How much is being spent in touting, puffing, advertising, printing, office rent, and expenses? That is, putting the question in a convenient form, what amount is spent in "commission and expenses of management" for every £1 of premium received?

If these facts are not satisfactory, we need go no further. If, however, they be satisfactory, we may proceed to ascertain those of lesser magnitude, the answers to most of which will not be found in the Blue-books.

Thirdly, how long has the office been established? Is the method of distributing the bonus good; or does that method offer the intending assured unusual advantages? What is the rate of premium? Has the office proprietors, or is it founded on mutual principles? If preference be given to an office having proprietors, to what extent do such proprietors participate in the profits, and by how much does the amount they receive exceed the amount of the current interest on the capital, or, to use other phraseology, what are the proprietors making out of the business, and the assured accordingly losing?

Fourthly, questions that may be asked *cæteris paribus*. Is the office large and popular? This will be shown by the amount of premiums. Does it employ agents and give commissions?

Of course, it were useless to find out these facts respecting an office, without having data with which to compare them. It is therefore necessary to extend our inquiries further and ascertain the practice of the best offices, by ascertaining similar facts respecting some half-dozen other offices of the best reputation. All the Parliamentary returns being to one form renders it possible to obtain the same facts respecting any office without difficulty. When the necessary facts as to these offices have been obtained, it is well, in order to counteract the effect of the opinion of any particular board of directors, or the circumstances of any special office, and to obtain a better standard for comparison, to strike averages of the answers to our calculations. It must not, however, be thought that the answers to our questions are exact. To make them so would require the most minute inspection of the office books and securities, and a most careful valuation of the liability on each policy. But the answers we are enabled to obtain are sufficiently approximate to make them practically reliable.

* Struts are wooden supports placed as a buttress against the weaker side of the pole to prevent it being pushed over.

† Stays are strands or ropes of stout iron wire fastened to the pole, and to an underground mass of stone, &c., for a similar purpose.

THE TELEPHONE FOR DIVERS.—Orders have been issued that telephones for the use of divers are to be supplied to Government ships carrying such men.—*Electrician*.

CYCLING AND CYCLES.

By "Axle."

THE art of cycling has bestowed a new faculty upon the human race. This is the opinion of the eminent physiologist, Dr. B. W. Richardson, expressed in an article recently contributed to *Longman's Magazine:* and in support of his assertion he has written, "By the simple machines, bicycles and tricycles, we are returning to first principles. We are endowing every person who can use these machines with a new and independent gift of progression; and to what extent this art will proceed in a quarter of a century, if it makes the same progress that it has made in the past twentieth part of a century, he would indeed be a bold man who would venture to predict."

Until quite recently, cycling has depended principally for the maintenance of its interest on the "sport" to be derived from its pursuit, but we are now becoming alive to the fact that it is to be viewed from much higher standpoints—those of its health-giving qualities and its usefulness as a medium for rapid conveyance. As regards the former, much has been said and written both for and against. Says Dr. Richardson — and he has devoted considerable attention to the subject —"I really know of nothing that has been so good for health." In the second case, there is no doubt that the cycle—more especially the tricycle—may be largely utilised as a "carrier." Our contemporary the *Standard* is already employing the tricycle in this capacity—for the delivery of newspapers within the metropolitan area,—and the vermilion-painted "carriers" used for this purpose are now familiar objects in the London streets. And obviously, in this connection, there are manifold spheres for the useful employment of the three-wheeled machine.

Regarded as a pastime, cycling is not of such recent introduction as many may be led to suppose, for as far back as 1769 we find recorded a description of a vehicle to be propelled by the action of its rider; and later, about the year 1820, a contrivance popularly known as the "dandy-horse" (inasmuch as it was principally patronised by the dandies of the period) was the subject of much public caricature and criticism.

It would be superfluous to attempt any detailed description of these early machines; and it will suffice to say that the "dandy-horse" was constructed with two wheels, one at either end of a horizontal bar, in the centre of which the seat was placed at such a height as to enable the rider to touch the ground with his toes without alighting, and by a similar action, as in walking, to propel the machine along. A cross-bar was fitted just in front of the seat, upon which the rider leaned with his hands; and a simple steering arrangement was attached. With practice, it was possible to travel upon this primitive bicycle at a fairly rapid pace; but the vibration consequent upon its use was, as may be imagined, considerable, and not at all calculated to impress a rider with the idea of comfortable progression. For this reason, and also, perhaps, on account of the ludicrous appearance presented by a person when mounted upon the machine—which called forth the ridicule alluded to above —the "dandy-horse" soon fell into desuetude; and though various improvements were from time to time attempted, nothing of importance was accomplished until 1856, in which year the afterwards famous "Velocipedes" began to attract considerable attention.

These velocipedes were constructed to carry one or two persons (and in some cases even three, *à la* tandem), and were made principally of wood, with light wooden wheels, iron-tyred. Their axles were cranked, and connected with the pedals by an iron rod, and were sometimes jointed to the front part of the machine, thus producing a lever action somewhat suggestive of that of the itinerant scissor-grinder's apparatus. In other cases, propulsion was secured by a "backwards and forwards" motion of a lever handle. The seats of these machines were usually commodious—and correspondingly cumbersome—saddles for seating purposes not having then been adopted. On level roads a very fair rate of travelling could be obtained with velocipedes, but great physical exertion was required to drive them up hill, in consequence of their weight, which was nearly double that of machines of the present day. Indeed, it was found more prudent, and certainly less fatiguing, to dismount and push them up-hill, and even this was no mean task. And the vibration, too, was scarcely less diminished than in the case of the "dandy-horse." Long-distance records were not then, we need hardly say, often to be met with, twenty miles being considered a very respectable day's journey.

Eventually a new era in cycling was marked by the introduction of a bicycle (the forerunner of the perfect machines of to-day) which was considered at the time to be an extraordinary improvement, though now—being in possession of such marvels of completeness as were recently exhibited at the Stanley Show—we contemptuously speak of it as the "bone-shaker." It was much lighter in weight than its predecessors, and the saddle was placed in the "backbone," between the fore and hind wheels; these more nearly approached each other in size than those of recent manufacture. As we have said, the "bone-shaker" was considered to be a great success, and when the initial difficulty of maintaining their balance had been overcome by riders, the machine made rapid progress in favour. Long distances could be accomplished upon them with comparative ease; and bicycle races became of frequent occurrence. One of the earliest of these was witnessed by the writer of this article, at a charming West of England watering-place. The occasion was a public holiday, and the race formed a leading feature of the sports promoted for the amusement of the holiday visitors, who, with the townsfolk, mustered in large numbers, great curiosity having been aroused by the novelty of the contest. The course was through the principal thoroughfares of the town, and as lap after lap was finished, the excitement of the onlookers became intense, and culminated in irrepressible enthusiasm as the riders approached the winning-post suffering from evident exhaustion. Many of the competitors were obliged to withdraw from the contest long before the finish, the race being, by reason of the clumsy and weighty construction of the machines, much more a test of stamina than of speed.

The foregoing incident is related merely to show that even in years long past cycling was a subject of general interest to the public; and from the time of the advent of such competitions, it has been regarded as almost the principal recreation of the time. Year by year, some improvement has been introduced, the result of scientific study, inventive genius, and patient research. The great desideratum was a maximum of speed to be obtained with the least possible exertion in riding, and to accomplish this it was necessary largely to reduce the weight of the machines without imperilling the safety of the riders. The heavy wooden wheels with iron tyres were discarded, and replaced in 1867 by a new style of wheel—originally the invention of a Frenchman—lightly constructed of metal, and tyred with india-rubber in order to minimise the objectionable "bone-shaking." One of the first of these wheels manufactured is still to be seen at the works of the Coventry Machinists Company, where it has been preserved as a specimen of the first step towards the perfection of cycles.

It has only been during the past eight or ten years, how-

ever, that anything like the eager competition at present evinced by manufacturers has arisen; and this has been occasioned by the really scientific knowledge of motive-power displayed by some of the more enthusiastic devotees of "the road." Countless improvements have been effected, even in the smallest details, all of which tend to the comfort of the cyclist when riding. It would be invidious to particularise either of the many manufacturers who have lent their aid to the furtherance of this end; but we hope occasionally to be able to describe some of these inventions in the pages of KNOWLEDGE, that our many readers interested in the subject may be afforded the opportunity of judging for themselves which, for their own requirements, is the most suitable machine.

HYMN ON THE UNIVERSE.

I.

ROLL on, thou sun! for ever roll,
Thou giant, rushing through the heaven!
Creation's wonder; nature's soul;
Thy golden wheels by angels driven.
The planets die without thy blaze;
And cherubim, with stardrop wing,
Float in thy diamond-sparkling rays,
Thou brightest emblem of their King!

II.

Roll, lovely earth! and still roll on,
With ocean's azure beauty bound;
While one sweet star, the pearly moon,
Pursues thee through the blue profound;
And angels, with delighted eyes,
Behold thy tints of mount and stream,
From the high walls of Paradise,
Swift wheeling, like a glorious dream.

III.

Roll, planets! on your dazzling road,
For ever sweeping round the sun;
What eye beheld when first ye glowed?
What eye shall see your courses done?
Roll in your solemn majesty
Ye deathless splendours of the skies;
High altars from which angels see
The incense of creation rise.

IV.

Roll, comets! and ye million stars;
Ye that through boundless nature roam;
Ye monarchs on your flame-wing cars;
Tell us in what more glorious dome—
What orb to which your pomps are dim;
What kingdom but by angels trod—
Tell us where swells the eternal hymn
Around His throne, where dwells your God?

GOETHE.

[It is earnestly requested that the insertion of this poem be not regarded as an occasion for critical comments by those who, chancing to have no poesy in their souls, are apt to regard poetic fancies as matters for prosaic argument. The glowing thoughts of the poet must be "taken warm." —R. P.]

OCCULTATIONS IN A THREE-INCH TELESCOPE.

BY A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

THERE are few more curious, instructive, nay, even startling, sights in the heavens than the Occultation of a fixed star, or (more rarely) of a planet by the Moon. When this occurs at the dark limb of our satellite, its suddenness is such as not infrequently to extort an exclamation from, as it invariably causes a start by, the observer who witnesses it for the first time. The Moon, as everybody knows, completes a sidereal revolution round the earth in about 27·32 days; in other words, that period elapses from the time of her quitting any given star to her return to it again. It may be worth while to mention incidentally that while the Moon has thus been travelling in an easterly direction through the sky, the Sun has also (apparently) been moving, much more slowly, in the same direction; so that if we assume that the Sun and Moon are in conjunction (the "New Moon" of the Almanacs), at the end of the 27·32 days the Moon will not have overtaken him; in fact, she will have to go on for 2·21 days before she comes up with him, and it is New Moon again. It is this period of 29·53 days which forms the Lunar or Synodical (Greek, Σύνοδος, a meeting) Month of the books on astronomy. In thus describing her monthly path over the celestial vault, it is quite obvious that she must pass between us and such stars as lie in her course; the stars being—for our present purpose—at an infinite distance, while she is only some 239,000 miles from us. Her orbit is, however, very far indeed from being a fixed circle in the sky. Its mean inclination to the Ecliptic is about 5° 9′; but its Nodes (the points where it cuts the Ecliptic, or plane of the earth's orbit) are perpetually shifting. The Moon's perigee, or nearest point to the earth, is shifting, and, in fact, to put it shortly, at the end of any month the Moon does not return accurately to that point of the sky from which she set out. Were the path of the Moon a definite and unalterable one in the heavens, she would, of course, occult the same stars over and over again, month after month. As a matter of fact, she only does this, and that but approximately, after 223 lunations—a period known to the Chaldæans of old as the Saros. Very well, then, travelling thus as we have said from west to east, her eastern limb is, of course, the leading one, or that which covers, hides, or occults the objects lying in her path. From New Moon to Full Moon this limb is unilluminated, and the effect of the extremely sudden extinction of a star when the dark limb hides it is, as we began by saying, of an absolutely startling character. "In a moment, in the twinkling of an eye," the star which shone as a brilliant point in the sky is blotted out; and its place seemingly knows it no more until it reappears from behind the opposite or illuminated edge of the Moon. After Full Moon, of course, the eastern limb is illuminated, so that the disappearance takes place at the bright edge, and the star on its reappearance starts instantaneously from behind the dark limb. A few days on either side of New Moon, when the dark limb is visible by earthshine—or, in the popular form of expression, we can see the Old Moon in the New Moon's arms—a new charm is added to the spectacle of an Occultation, inasmuch as before Full Moon the faintly-lighted dark limb can be actually seen approaching the star which it is soon to obliterate. Now it fortunately happens that Occultations are phenomena peculiarly within the range and capability of a three-inch telescope. Moreover, should the owner of such an instrument also happen to possess

a trustworthy chronometer or regulator, he may not only derive great personal pleasure and amusement from the observation of the phenomena of which we are treating, but may render real and enduring service to science by the publication of his observations of the Occultations which are predicted in the *Nautical Almanac*. It may encourage the student and young observer to be assured that observations of Occultations made with a three-inch telescope and an accurate chronometer, may be of real service in correcting the lunar tables, and the theory generally. As an illustration of the preceding remarks, we will take the Occultation of λ Geminorum by the Moon, which happened on Thursday, March 6. Our sketch represents the aspect of affairs at 10h. 10m. 4s. p.m., at the instant of the star's disappearance at the Moon's dark limb, as seen in a three-inch telescope armed with a Huyghenian (inverting) eye-piece magnifying 80 diameters.

Occultation of λ Geminorum by the Moon, March 6, 1884, power 80.

The reappearance happened at a point in the bright limb which may be found in the engraving by opening the legs of a pair of compasses 0·55 in., and placing one leg on the lowest illuminated point of the Moon's disc; then will the other leg cut the bright limb at the spot at which it took place. It occurred about 11h. 12m. 9s. p.m. Let us now turn to p. 435 of the *Nautical Almanac* for the current year, and see in what form this phenomenon is there predicted, with a view to explaining and utilising such prediction for the information and instruction of the student.

OCCULTATIONS VISIBLE AT GREENWICH.

Month and Day.	Star's Name.	Magnitude.	Disappearance.				Reappearance.			
			Sidereal Time.	Mean Time.	Angle from N Point.	Angle from Vertex.	Sidereal Time.	Mean Time.	Angle from N Point.	Angle from Vertex.
			h.m.	h.m.	°	°	h.m.	h.m.	°	°
Mar. 6	λ Geminorum	3½	9 10	10 10	107	134	10 11	11 11	226	262

*** The angles are reckoned towards the right hand round the circumference of the Moon's image, as seen in an inverting telescope.

The first column gives the date; the second, the star's name; the third, its magnitude; the fourth, the sidereal time—or that shown by an ordinary observatory clock—at which the disappearance takes place; and the fifth, the corresponding instant of mean solar time at which the star vanishes. The sixth and seventh columns require more detailed explanation. The "North Point" of the table is that point of the Moon's limb cut by a circle passing through the North Pole and the Moon's centre. It is in real truth the *South* point of the Lunar limb; but it is uppermost in the field of view and is known technically as the N. Point. From this, angles are measured right round the Moon's circumference, in the direction in which the hands of a watch move. Our second figure will illustrate this.

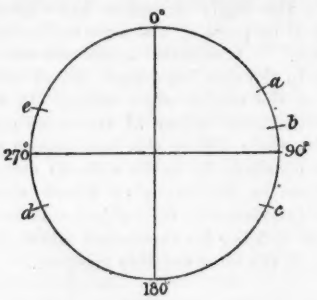


Fig. 2.

This will render the mode of measurement intelligible at a glance. Here, evidently, 0° is the Moon's (technical) N Point, and the measurement of angles from it is indicated by the figures. Suppose, for example, that we found from the tables that the angle from N Point of some star at disappearance was 60°; then should we know that it would be occulted at the point marked *a* in our diagram. Were the angle 79°, it would disappear at *b*, 116° at *c*, and so on. Were, on the other hand, the angle of reappearance given as 245° from N Point, *d* would be the point in the limb from which it would emerge, as would *e* were such angle 292°. These "angles from North Point" are employed with telescopes mounted equatorially (Vol. I. p. 201). When the observer has only an altazimuth mounting (*loc. cit.*), as is usually the case with a three-inch telescope, the angles must be measured from the Moon's vertex. This in effect is the point in her limb at the top (in an inverting telescope) cut by a plumb-line passing through her centre. The method of deducing the angle from the vertex, that from the North Point being given, will be found (in Letter 277) on p. 210 of Vol. III. of KNOWLEDGE. We have so far spoken as though the disappearance of stars was in all cases instantaneous, and so, as far as our own experience goes, it is. Other observers, though, have seen the very curious phenomenon of the apparent projection of the star on to the (almost invariably bright) limb of the Moon. It is a noteworthy fact that this curious appearance has been practically confined to red stars, like Aldebaran; but this goes a very little way in helping towards a solution of so anomalous an effect. The Occultations of planets are comparatively rare phenomena; and should be sedulously watched whenever they occur. An Occultation of Venus occurred about 3 o'clock in the afternoon on February 29; but no intimation or prediction of it whatever was given in the *Nautical Almanac!* Occultations of Saturn and Jupiter afford delightful spectacles to the observer; the extreme sharpness of their superficial detail, where actually in contact with the Moon's limb, entirely negativing any suspicion of a lunar atmosphere. Irrespectively altogether, however, of the mere beauty and interest of the phenomena of Lunar Occultations, and their entire suitability for observation with our instrumental means, we would, in conclusion, once more insist on their scientific value. Made simultaneously at stations, the longitude of one of which is well determined, they afford excellent (if somewhat operose) means of deducing that of the other. Moreover, as we have before said, if the student be the possessor of a chronometer indicating accurate Greenwich Mean Time, he may by his own unaided exertions render real help towards the improvement of the Lunar Theory, and perchance earn a niche in the Temple of Fame in days yet to come.

THE ORNITHORHYNCHUS.

(*Ornithorhynchus paradoxus.*)

By L. P. Gratacap.

THIS interesting animal has proved both a perplexity and a delight to naturalists. Its little body is so curiously constructed as to remind the student of structural affinities in animals of three types of life—the mammals, birds, and reptiles. It undoubtedly belongs to the former, but it enters that class at its lowest point, and brings along with it features and reminiscences of more degraded forms than itself. It is a welcome gift to the evolutionist, and he has not been loth to emphasise every indication it gives of its intermediate and connective character. In spite of these suggestive resemblances the ornithorhynchus is essentially *mammalian*, though holding the humblest position in this group. With its singular ally, the *Echidna*—the porcupine—anteater—it forms the division of *Ornithodelphia*, and is especially characterised by a strange provision in its economy, by which the fæces and young are extruded through the same passage, as the spacious cloaca is common to the rectum, genital, and urinary organs. Hence, the technical appellation of *Monotemata*.

The features which ally it to the amphibia or reptiles are chiefly found in the skeleton, and are the following, among others less obvious:—A projection of the second neck vertebra, called the "odontoid process," remains for a long time disconnected from the vertebra itself, upon which it is normally soldered by a long growth between the surfaces. Some of the cervical ribs in a similar way remain free.

The coracoid bone, which in man is a process only of the scapula, or shoulder-blade, but in birds is a separate bone, as also in reptiles, and which is a large bone in this animal, articulates with the sternum or breast-bone directly. This is a positive amphibian and avian feature. There is an ossification in front of this bone, called the *epicoracoid*, which resembles a similar portion of the reptilian frame. In these

Skeleton of the Ornithorhynchus.

mammals alone there is a T-shaped bone supporting the shoulder-blades or clavicles. The *acetabulum*, or cavity, into which the head of the femur is thrust as in a socket,

remains unossified at its centre, thus resembling birds and crocodiles. Other points in its anatomy and physiology strongly suggest its indeterminate and dependent character, but its nature and functions place it beyond appeal among the mammals as a class.

The ornithorhynchus, by its grotesque union of the externals of a duck, beaver, and mole, its restricted range geographically, and the singular and unwarranted tales told of its habits by natives of Australia, has always formed a natural curiosity, and been regarded with mingled feelings of amusement and astonishment. The first skin of this animal sent to England presented such anomalous features that it was regarded as the playful hoax of some ingenious collector. A duck's bill and a mole's body presented a zoological complication which at first could not be considered seriously.

The ornithorhynchus is about the size of its congener the echidna, having an average length of 50 cm. (1 ft 7·6 in.), 12 of which measure the normal length of its tail. The males are larger than the females. The flattened body is not dissimilar in some aspects to that of the beaver or fish otter. The bones are short, each foot or paw is provided with five claws, which are webbed, and this integument on the front feet is developed to such a degree as to extend beyond the extremities of the claws; it folds or draws back at the will of the creature, permitting it to use its serviceable talons or nails for digging and excavating. The short hinder feet are turned backward, and are usually placed in that position, and the nails, which are longer and sharper than those of the front feet, are similarly bent backward. In the males, above the toes of the hinder feet, there is a spur which admits of considerable movement. The tail is flat, broad, abruptly terminated, and in the younger specimens provided plentifully with hair, which disappears with age.

The head is quite flat, and forms the most distinguishing feature in its appearance. It is small and furnished with a duck bill, at the base of which a leathery apron-like expansion is developed, which acts as a shield, protecting the eyes when the animal burrows in the ground, and guarding the fine fur behind it from the slime of the muddy bottoms where it searches for its food. The jaws are prolonged forward, and carry no teeth; the margins of the duck-like bill are sheathed with horn and crossed with horny plates. The tongue is fleshy, armed with horny carunculations, and terminated at its base by a ball-like swelling which closes the throat. The eyes are small, and the barely noticeable ears, sunk in the head near the outer angle of the eyes, are closed at will. The fur on the upper surface of the animal is dark brown, sometimes reddish; it is composed of one set of long hairs which are somewhat stiff, and of another shorter growth of fine grey hairs, similar to the woolly coating of the seal. The fur on the breast and neck is silken and yellowish. The bill is black, spotted with light points, and is red at its extremity. The fur of the tail varies in colour, which has given rise to suspicions of different species, and in the younger individuals it is coated with fine and silver-white hairs, an almost unmistakable indication of immaturity.

The ornithorhynchus inhabits the still pools of streams where water-plants abound, and over whose serene expanse trees bend their shadowing branches. Here in pursuit of its amphibious existence, hunting the insects which haunt the water, grubbing around the esculent roots of plants, building its home, and eluding pursuit when the natives, who regard it as a delectable morsel, watch patiently for its appearance, spear in hand, upon the banks of the pond. The traveller who is fortunate enough to surprise these animals when actively engaged in their pursuit of food, must remain preternaturally still, if he wishes to enjoy the novel spectacle. If the water is clear and the light favourable, he will see them moving rapidly beneath the water, avidly inspecting the soft banks for beetles; they will rise to the surface every two or more minutes, again disappear to emerge later at a distant point. The slightest movement or noise is instantly detected, and the shy, strange animal is put to flight, and the chagrined spectator must endure a prolonged watch before it reappears.

The nest of the ornithorhynchus is located under ground, and is placed at the end of a long, underground, devious, passageway, which may be, in exceptional cases, 45 feet long, although more usually 10 feet. This avenue of approach is strewn with dead leaves, as is also the kettle-like hole in which it ends. The homes of the ornithorhynchus are entered by two passages, one above and the other below the surface of the water. Almost invariably the nest is placed beyond danger of the infiltrating water of the highest tides.

At rest the animal assumes various positions; the two most familiar are shown in the accompanying illustration. It rolls itself up in a ball, with its fore feet tucked under its bill, its hind feet pressed tightly over it, and its tail drawn down over all, or else its lies on its back, with its four feet stretched upward in languid delight.

The natives aver that the female lays eggs, and that the male inflicts poisonous wounds with its spur, both of which stories, formerly received with credulity, have been abundantly disproved.—*Scientific American.*

RIVER ACTION ON LAND.*

AMONG the many important and complicated questions with which the practical hydraulic engineer has to deal, there are few more important and few perhaps more complicated than that of the discharge of water through open channels. Naturally, therefore, it has engaged the attention of engineers from an early period, and the formulæ laid down on the subject by Dubuat, Eytelwein, and others date from centuries ago. Their experiments, however, were mostly conducted on channels of very small section; and it has long been recognised that to extend these to natural rivers, or even to channels of different dimensions, is to commit a grave error. Numerous formulæ have since been introduced in the hopes of improving on the results of these early experimenters. Turning, for instance, to the well-known pages of Molesworth's "Pocket-book," we find that the mere list of such formulæ occupies almost a page of small print, and that at least twenty authorities are there mentioned by name as having contributed to the subject. But as a striking commentary on their success a small table is given at the bottom of the same page, where four cases of actual discharge, varying from 24 to over 1,000,000 cubic feet per second, are compared with the calculated results for the same conditions, derived from nine approved rules on the subject. The differences are very striking. In some cases they amount to nearly 100 per cent., and in many cases to at least 50 per cent. Nor is this a solitary instance. Major Cunningham, in his recent work on the Roorkee hydraulic experiments, arrives at very much the same result. Again, in papers published in our issue of Nov. 1, 1872, p. 293, the results given in the classical experiments of D'Arcy and Bazin are compared with the calculated results derived from several of these same formulæ, the object being to test the value

* From the *Engineer*.

of the formulæ obtained from theoretical considerations by the late Canon Moseley. Any one who will turn back to these figures will see divergences startling enough to prove that we must at least use considerable caution in adopting any formula for such purposes, and yet the conditions were here unusually favourable, as the experiments were carried on, not in the difficult and irregular channel of an actual river, but in an artificial conduit specially prepared for the purpose. In spite of these facts, which it is impossible to deny, nothing is more common that to see the most confident and offhand statements made as to the discharge of all sorts of rivers under all sorts of circumstances. To take one example, derived from the science of geology. It is frequently stated in geological works—as, for instance, by Mr. Alfred Wallace, and by Professor Geikie in his late admirable "Manual of Geology"—that a certain definite thickness of earth is removed from the surface of the land every year by the rivers which flow through it, and is by them emptied into the sea. The actual thickness in thousandths of an inch per annum, is given with all the confidence of an approved scientific fact. We have never been able to trace this statement to its original source; but we have no hesitation, as practical engineers, in pronouncing it to be altogether visionary. It is worth while to consider for a few moments what data would be requisite before such a figure could be laid down, even to the very roughest degree of approximation.

Of course, the only possible method of determining it lies in measuring, by some mode or other, the quantity of mud—or silt, to use the most general term—which all the rivers, say, of this country, carry into the sea during the course of a single year. What would any engineer do if he was given the commission of carrying out such an inquiry? He would have, in the first instance, to ascertain the mean annual discharge of every river or stream emptying itself into the tidal estuaries of Great Britain. It would be worse than useless to conduct observations upon these estuaries themselves, because it is perfectly well-known that the silt suspended in such waters at any moment gives no evidence whatever as to the amount of silt which is conveyed into these estuaries from the uplands during each year or day. Therefore, it would be vain, for instance, to compute the silt carried down by the Thames from observations taken below Teddington Weir; we must examine the Thames itself above that weir, and all the streams which enter into it lower down must be subjected to a separate investigation. Let us consider next what will be necessary in the case of any one of those streams.

In the first place, our engineer must have the means of measuring its discharge with sufficient accuracy on any particular occasion he may desire. We have said enough already to show that this is an exceedingly difficult matter, that he will be unwise if he relies even upon the best of the dozen or so of formulæ amongst which he may take his choice; and that he is in duty bound to make an accurate determination in the case of each river, in order to take full account of local conditions. Let us assume that he has done so, and that he is thus able by a series of observations to ascertain with fair correctness the discharge taking place on any particular day. It is obvious that to measure this discharge for a single day only would be utterly futile. During a winter flood many rivers, even in England, will send down 50 to 100 times the water in an hour, compared to that which dribbles over their bottom towards the end of a summer drought. Our engineer must, therefore, measure all the great floods which occur during the year he has selected, and must also take a large number of measurements both in the wet and dry seasons. This done, he must search carefully the meteorological records of the district—if he can find any—in order to ascertain whether this particular year may be taken as a fair average example, and if not, he must make such addition to, or subtraction from, his results, as his own judgment shall direct him. Failing this, he will have no resource but to renew his observations from year to year, until, in the lapse, say, of a generation, a true average can be struck. At the end of this time, provided there are no indications of a progressive change in the climate and rainfall, he may fairly be allowed to state what the mean annual discharge of this particular stream may be.

But his work is only begun after all; it is not the quantity of water discharged which he is in search of, but the quantity of silt. He has to determine not only the number of cubic feet of water which have flowed through his channel in a particular year, or a particular half-century, but how much solid matter each of these cubic feet held in suspension while it passed. To do this, it will by no means suffice to pick up a bucketful every time that he makes an observation, have it carefully evaporated, and weigh the residue which remains. We know scarcely anything of the laws of distribution of suspended matter within the waters of a stream. It may, indeed, be assumed that the quantity per cubic foot will be larger towards the bottom than towards the top of the current; but the law according to which this varies is quite unknown. All that we can be sure of is that this law itself will vary, and probably very largely, with the depth of the current, with its velocity, with the contour of the bottom, with the material of which that bottom is composed, and probably with many other local circumstances. It will not do, therefore, to trust to anything less than the collection of a large number of samples, say 50 to 100, from all parts of the cross-section, on each occasion when the discharge is measured, or, at any rate, on each occasion when the river is in any abnormal condition. Suppose this to be done, and the weight of silt per cubic foot at each of those places to be ascertained by the slow method of evaporation and weighing, then it will by no means do to strike an average of all the fifty, and multiply this by the number of cubic feet discharged per hour; for the velocities at different parts of the cross-section are very different, and this will clearly modify the results. Thus, if the velocity at the bottom is half that at the top, whilst the weight of silt per cubic foot is double, it will be seen that the amount of silt carried down per square foot of area at the bottom and at the surface will be really the same. Hence, we must know the average velocity in each of the fifty divisions, say, into which our cross-section has been partitioned off for convenience, and we must also know the average weight of silt per cubic foot corresponding to that division. Multiplying together each pair in these two series of numbers, and adding the products thus obtained, we shall arrive at some sort of approximation towards the quantity of silt which our stream was carrying down per hour, on the day when this particular observation was taken, and the same process will have to be repeated for every one of the separate observations which have been described as being necessary in order to solve the problem of mean annual discharge. The experiments must necessarily be conducted over a considerable number of years, because it is quite possible that the effects of drainage, denudation, or other causes, may produce a progressive increase or diminution of the average quantity of silt borne down, and of this it will be necessary to take account.

So much for our single river; we have now to perform

the same task for every other river and stream, from the largest to the smallest, which empties into a tideway round the coast of our island. The smaller streams will, no doubt, give less trouble than the larger, but they must by no means be neglected. We have no means at hand of estimating the number of such streams, but assuming that there is merely one for each mile of coast, it will be evident that it will be considerably over 1,000. Supposing, however, that the whole of these have been gauged, and the discharge of silt calculated in the manner just described, we may perhaps imagine that our engineer's task is at an end. Not a bit of it. He has to ascertain not merely how much solid matter has been carried into the sea per annum, but how thick a layer of solid matter has been subtracted from the land. Now the quantity of the solid matter upon the land is being added to every year by the operation of certain very obvious causes, such as the falling of leaves, the decay of plants and animals, the application of foreign manures, &c. Possibly some one may object that organisms, whether plants or animals, can only build up their substance from materials already existing in the earth; but a moment's reflection will show that a large part of their substance is derived either from water or from air. It is precisely this decaying organic matter, lying, as it does, at or near the surface of the ground, which will be washed off in the greatest proportion by rain and by rills, and will so find its way into the rivers and thence to the sea. Therefore, our engineer must of necessity do one of two things: he must either analyse carefully every ounce of silt recovered in his observations in order to ascertain beyond a doubt, first, how much of it is due to organic and how much to inorganic matter, secondly, how much organic matter was derived from the earth, and how much from air and water; or, failing this, he must by some means or other calculate the whole volume of matter which has been added to the earth by the causes above mentioned, he must measure the quantity which remains at the end of that period, and he must subtract the difference—or rather the difference less that part of it which is due primarily to inorganic constituents—from the total amount which he has already ascertained to form the burden of the rivers as they fall into the sea.

We have, perhaps, said enough to show, however faintly and inadequately, the nature of the task which an engineer would have before him if he were set to ascertain the correctness of the figure which geologists quote so confidently, viz., the thickness of the layer of soil which is removed annually from our British Isles by the operation of what is called sub-aerial waste. A feeling of longing and regret steals over us as we close the record. What a pity that the determination of this figure is not a matter of paramount national importance, to be settled at any cost! and what a pity that we ourseves are not given the responsible task of settling it! It would resemble one of those magnificent Chancery suits which an attorney of the old school was wont to regard with so much complacency and satisfaction; a suit which he could slowly administer during his life-time, and hand on to his children with his blessing on his death-bed, certain that it would remain as a sure and comfortable source of income to them and their children yet unborn. And yet geologists quote this figure with perfect confidence, as if it was known to the 10,000th of an inch. They do more—they calculate on this basis the number of years which will elapse before Great Britain becomes a dead level, totally forgetting that the diminution of slope all over the country will wholly change the conditions of the problem. They do this in the name of science, and in the next breath inform us that science is measurement!

Editorial Gossip.

Some of the newspaper accounts of the boat-race were scarcely less absurd than the marvellous accounts of the practising. One paper finds that from Barnes Bridge Oxford had two "passengers" at least,—that is, men not pulling their weight, and stroke shirking by not covering his blade. Cambridge, by the same accounts, rowed this part of the distance magnificently. Rather an astounding feat for five Oxford men and a work-shirking stroke to row a boat with two "passengers" from Barnes Bridge to the "Ship" behind a splendidly-rowed eight, without losing more than half a length, if so much! I was at the "White Hart," Mortlake, and had the Oxford boat under my eye for some two hundred yards, and I venture to say that the whole distance was most honestly and pluckily rowed by every man in the boat. Not a man shirked the tenth part of a stroke.

All know that the Christian Easter is an adaptation, with altered symbolisations, from the Jewish Passover. It is nearly as certain that the Jewish Passover was an adaptation with changed meaning from an Egyptian festival. Even for the Sabbath Moses found a meaning connected with the departure from Egypt (Deuteronomy v., 15), and the Jewish observance of the Sabbath is known to be a relic of Egyptian ceremonial. But about the feasts of the Passover and of Tabernacles, there can be no more doubt than about the sunrise and sunset observances, and the new moon festivals, that they came originally from a race worshipping the heavenly bodies, as every race of man has worshipped those impressive orbs in one part or another of its progress from savagery to civilisation. The great Sun-God was for six months below the equator, for as many above that great circle of the heavens; Day was for half the year weaker than Night, for half the year stronger; the Sun-God Herakles or Samson was for six months triumphant, for six months held in bondage and shorn of the rays which are his might. That the time when he descends below the equator should still be observed as a time of lamentation by the Jews, though they are no longer sun-worshippers, is less wonderful, considering the conservatism of the human race, than that Christians should somehow have gotten rid of that particular fast which the Jews were enjoined to observe with mourning,—so strongly enjoined that whosoever did not afflict himself was to be slain. The other festival held of old in honour of the triumphant ascent of the Sun-God from the dark depths below the equator, the Jews retained as symbolising a passover of a different sort, while it has come down to our time and to Christian nations still further modified in form,—though still typifying triumphant return from darkness and death. Strangely enough the name we have for the festival (which has the same relation to rising that the word East has) came to us more directly, being the Saxon name for the festival of the Sun-God's return to the ascendant part of his annual career.

Reade, Trollope, Dickens, Thackeray,—within a score of years, these four have passed from among those for whom they afforded so much pure pleasure, among whom they wrought so much good. Looking around at the world as we find it, considering its sorrows and its joys and what share each one of us has or may have in contributing to one or the other, where shall we find four others of those who speak our tongue who have done more for good within the last half-century than these four, who lived in our midst untitled and unrewarded save by the direct product of their

very own labours? Truly when we consider the actual fruits of the actions of men, how paltry, where they are not absolutely mischievous, seem the doings of many who are regarded as the great people of the earth. Taking the balance of all the deeds of kings and potentates, warriors and statesmen, within the last two thousand years, on the one hand duly counting such good results as they have achieved but on the other estimating aright the multitudinous miseries they have wrought, it is not too much to say that the least of the four named above, the least among discoverers and inventors, the least among poets, historians, philosophers, and artists, has done work which outweighs all the deeds of war and policy which men in their weakness delight to applaud.

The death of such men as Charles Reade, even though he had reached the three-score years and ten allotted to man, and had done the best of his work ten years since, is a real loss to the world. He will not be buried with the pomp and ceremony which pretends to be national mourning, but he will be mourned and lamented by hundreds of thousands who personally never knew him,—and for the just reason that he did good which could be felt and measured. No compliments of mourning are needed and none need be uttered when such men die. No national tokens of grief need be ordered, and none need ape an unfelt sorrow. We need hang no heraldic blazonry over the tomb of the worthy dead, no tokens of qualities with which he was not gifted or of deeds which he never wrought. None need see the dignity of human nature degraded by false shows or pretended sorrow. The name of the man and the memory of what the man did, suffice. As he said himself of a great man dead, whose deeds and words live (centuries after his contemporaries the bluff and brutal Henry, the splendid and shallow Francis, the grand and grasping Charles, ended their conspicuous but worthless careers in their native dust and ashes),—"the words of genius are not born to die; their immediate work upon mankind fulfilled they may seem to lie torpid; but at each fresh shower of intelligence Time pours upon their students they prove their immortal race; they revive, they spring from the dust of great libraries; they bud, they flower, they fruit, they seed, from generation to generation and from age to age."

Eastward Ho! is the title of a new sixpenny monthly magazine, of which the first number is to appear on April 25. It will form a medium for conveying to the upper classes the views and suggestions upon social questions of those interested in the progress and well-being of the people. Among its earliest contributors will be the Bishop of Bedford, G. Manville Fenn, G. R. Sims, Rev. Brooke Lambert, W. G. Wills, &c. It will be published by Henry G. Davies, 73, Ludgate-hill, E.C.

The report on the mineral statistics of the United Kingdom for 1882, prepared by the Inspectors of Mines, shows that during 1882 156,499,977 tons of coal were raised, of the value, at the mine, of £44,118,409; 226 oz. of gold, realising £863 at the average market price; and 372,544 oz. of silver, of the value of £80,426. The quantity of iron ore raised was 18,031,957 tons, representing a value of £5,779,285 at the mine, and of metal contained in this ore 6,513,281 tons, of the value of £18,237,186. The total value of the minerals raised in 1882 was £54,879,507, and of the metal contained in the ores, £20,558,050.

"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

All Remittances, Cheques, and Post Office Orders should be made payable to Messrs. Wyman & Sons.

The Editor is not responsible for the opinions of correspondents.

No communications are answered by post, even though stamped and directed envelope be enclosed.

FRUIT TREES IN IRELAND.

[1191]—"A Resident Irishman" puts forth in No. 1186 a curious theory to account for the absence of fruit trees in the small farms of Ireland. He says that the *vera causa* is the "ungenial climate."

If so, the climate must be curiously variable, for on the very same estates it happens generally that fruit trees flourish in the "demesne" (*i.e.*, the grounds surrounding the house) of the landlord, but not on the holdings of his tenants. It also happens that fruit trees grow well enough on the tenant farms of exceptional landlords, such as the Duke of Devonshire, Earl Fitzwilliam, and others, whose natural sense of justice anticipated those provisions of the Land Act which, I expect, will transform the physical as well as the moral aspect of the most wretched parts of Ireland by enabling the worker to reap the fruits of his own industry.

Everybody, resident or non-resident, who knows anything at all concerning the climate of Ireland, is aware that it is more suitable for the growth of ordinary fruit trees, than that of either England, Wales, or Scotland; that all the Western side, from Cape Clear to Malin Head, has a climate which for geniality and equability has no rival in corresponding latitudes in any part of the world. It is similar to that of South Devon, Cornwall, and the Channel Islands, from which we obtain so much of our best and earliest fruit supplies. It is a climate tempered throughout the year by the unfettered floes of the Gulf Stream, which washes the whole of the Western coast.

Every botanist knows that plants are growing wild and luxuriantly on the hill slopes of this coast, which perish when transplanted to any part of England. One example will illustrate this. When I was on a visit to my friend, Professor Allman, of Galway, at his charming summer retreat in Roundstone Bay, Connemara, we strolled on the hills and gathered specimens of the "Connemara heath" (*Erica cantabrica* of Linnæus, now named *dabecia polifolia*, as it is not a true heath, though much like one). This is found nowhere else north of Spain, and every attempt to acclimatise it in England has failed. I planted some roots in a warm corner at Twickenham, but the first Twickenham winter killed them.

I shall never forget the avenue of fuchsias at Letterpack, the beautiful trees arching over the road like the nave of a Gothic cathedral, their crimson blossoms tinging the sunlight that glinted between them. In the grounds of Kylemore Castle, hard by, is one of the finest collections of exotic shrubs to be seen anywhere in Britain. Some of the Californian firs render the assertions of the "Resident Irishman" concerning the ungenial climate absolutely ridiculous. When I was there in 1877, and visited Mr. Mitchell Henry at the Castle, he told me that, in consequence of the remarkable mildness of the climate, he was able to make hedges of crimson fuchsias, which in four years grew thick enough to resist sheep. I saw these hedges in the course of growth, taking the place of the posts and wires, gorse hedges, and stone fences that had preceded them. Slips were then being rudely thrust into the ground, no further trouble being demanded for their propagation. I have heard other resident Irishmen assert that the general barrenness of the wilds of Connemara, which, if planted, would be a region of unrivalled scenic beauty, is due to climate. Mr. Mitchell Henry (an Englishman) has demonstrated the monstrous absurdity of this Irish notion by the luxuriant foliage with which he has surrounded his noble residence, and he has proved that the bogs, like the treeless wastes, are merely the results of shameful human

negligence; the negligence of landlords who will neither reclaim them with their own capital as he has done, nor give to the tenants that permanency of tenure that would induce them to do so.

The "Resident Irishman" jumps to a very false conclusion in supposing that I join in the denunciations of the Saxon. My explorations of Ireland, extending to every county in the island, have satisfied me that the Saxons have been, are, and will be yet, its true benefactors in developing the splendid natural resources of that shamefully neglected country.

He speaks of the provisions of the Act of 1870, but must surely know, as everybody else does, why they have been a dead letter. They provided that the poor tenant either should obtain the produce of his labour in planting trees, or otherwise improving his holding by going to law against his landlord, but provided no other machinery than the ordinary court for that purpose. My readers may imagine the case of a poor, potato-fed, ragged cottier, registering each of his gooseberry-bushes, and then going to law for their value against a territorial magnate who had a final appeal to the British House of Landlords. The resident Irishman must know how the Earl of Leitrim publicly announced his intention of defying and violating the law of 1870, and how he proved his feudal power over his tenants by commanding them to publicly and ostentatiously insult the Lord Lieutenant, and through him the Queen and the British Government; how he Boycotted (before Boycott) the Lord Lieutenant and his suite at Maume, in spite of legality, and how he drove them out of his dominions without bit or sup for horse or man. This was done to demonstrate his contempt for that same law which, according to the "Resident Irishman," any one of his poor tenants could enforce against him.

W. MATTIEU WILLIAMS.

INFINITY.

[1192]—On reading the letter 1160 in your paper signed "H.," I am reminded of Dr. Büchner's remarkable chapter on Infinity of Matter either in greatness or smallness, extent in space, and divisibility. Büchner met, in Germany, for his now famous work on "Force and Matter," with quite as much hostile criticism as Mr. Herbert Spencer and other great thinkers have met with in this country. As usual, the teachers of the religion of "love" and "good will towards all men," are to the fore in their demonstration. How different is their teaching from their practice!

His words on atoms are (p. 97 "English Translations"):—"We have no real notion of the thing we term atom; we know nothing of its size, form, composition, &c. No one has seen it. The speculative philosophers deny its existence; as they do not admit that a thing can exist which is no longer divisible. Thus neither observation or thought lead us, in regard to the minuteness of matter, to a point where we can stop; nor have we any hope that we shall ever reach that point."

He further says (p. 97):—"We can, then, only say that matter and the world are infinite in minuteness; and it is of little consequence if our intellect, which is always accustomed to find a limit, is offended at the idea."

Büchner's "Force and Matter" (p. 97):—"If, then, we can find no limit to minuteness, and are still less able to reach it in respect to magnitude, we must declare matter to be infinite in either direction, and incapable of limitation in time and space. If the laws of thought demonstrate an infinite divisibility of matter, and if it be further impossible to imagine a limited space or a nothing, it must be admitted that there is here a remarkable concordance of logical laws with the results of our scientific investigations." F. W. H.

SOLAR SPOTS.

[1193]—I have read your paper in *Longman's* with great interest, because it is something like certain thoughts I have entertained respecting the origin of the solar spots. But I was led to abandon the idea of eruptive action from the sun's real globe because of the long duration of many that have been seen. You state there is about four days' difference in the rotation of spots situated near the sun's equator and those nearest the poles. I apprehend this to be fatal to a theory attributing them to eruptions from a globe so distant as upwards of 100,000 miles, and very probably rotating at a greater speed than its outer envelope (a solid globe cannot rotate at both rates). Still, I must imagine them due to eruptive action from below, although that action is still far above the sun's solid globe.

The density and pressure of the solar atmosphere intervening between the photosphere and the globe must be something enormous. If of the nature of gas, it must be very different in composition to any known to us, and therefore speculation concerning it would lead to no certain result.

What about the *waste* constantly streaming (or ascending) from the surface of the globe into its atmosphere?

THOMAS AYERS.

[The greater part of such matter would return, and more matter would arrive from without than would be parted with. As for the varying rotation rates, they affect the cloud-laden region only. A spot would neither be originally, nor remain, vertically above the seat of disturbance.—R. P.]

GHOSTS AND GOBLINS.

[1194]—Although a disbeliever in everything supernatural, you will, perhaps, allow me to relate an incident which came within my experience, and which has some bearing on the interesting series of articles on "Ghosts and Goblins," appearing in KNOWLEDGE. Thirteen years ago I was a pupil in a Highland parish school in Inverness-shire. The school was not more than a hundred yards from a burying-ground, and two old maids resided in a thatched house quite near to the school-house. Both were regarded as model Christians in the district, and, as a consequence, were highly esteemed. Sometimes they visited the houses in the district, in the capacity of catechists, and took great pleasure in indoctrinating the boys and girls in the theology contained in the Westminster Confession of Faith. I recollect on one occasion, when one of them called at my father's house, being deputed to see her home. The night was pitch dark, and we had great difficulty in keeping the road. About half way I was walking along the centre of the road and she at the side. She all of a sudden rushed over to me and took a hold of my arm, and pulled me to the side, remarking, "Come out of the way, a funeral procession is passing." I replied that I could not see it, but she solemnly assured me that while I was speaking she was seeing the coffin carried shoulder high, and a number of men, many of whom she mentioned by name. "You will see," she said, "within three days a funeral procession pass along this way to the churchyard." I took a note of the names that she mentioned, with particulars of the dress they wore, and determined to test the accuracy or falsity of her story. The day after she had witnessed the procession of phantoms a boy was drowned while attempting to cross the river Dulvain, and his remains were interred in the burying-ground two days afterwards. I had the curiosity to go to the spot where the woman had seen the procession and watch it pass, and, strange to say, I saw the men she had enumerated, and attired in the dress which she described. I asked her afterwards about the supernatural power she seemed to possess, and she told me, with tears rolling down her cheeks, that it was the curse of her life. She could not go anywhere after nightfall without seeing ghosts and goblins. I have no explanation to offer, but I can testify to the accuracy of the account I have given,

J. CAMERON.

[1195]—Will the following, which happened to myself, be of any use in illustrating the causes of mysterious noises?

Many years ago, when we first took possession of our present home, a large country house, built in the reign of Queen Anne, my wife and I occupied a bedroom, the walls of which were panelled. After some time we became aware of a curious ticking noise, which we heard nearly every night soon after we had retired to rest—the sound was very like that produced by the beetle commonly called a "death-watch," but it was clearly not caused by that insect—it would last for a few minutes, and then cease; sometimes, but not always, recommencing after a short interval; occasionally several nights intervened without our hearing it, when, just as we were congratulating ourselves on having got rid of our torment, for such it was really becoming, it started afresh, and went on as before; but we could never hear it in the daytime.

At length the ticking became rather worrying, but being determined not to be driven out of our room, the only alternative was to make a serious effort to ascertain the cause, which was no easy matter; the room was large, and it was difficult to decide exactly the spot from which the sound proceeded—sometimes it seemed close to our bed, at other times in a distant part of the room—we were satisfied it was caused by neither an insect, mouse, or rat; at length, after much patience and careful listening, we fixed upon one particular panel as that from which the sound seemed to proceed.

The next morning I took my tools and cut a piece out, but on peering into the vacant space behind the panelling nothing was to be seen or heard. A three-foot rule was introduced, and, on withdrawing it, I felt sure I had caught the ghost. At the end of a stout line of cobweb was firmly secured a piece of hard mortar about the size of a pea! A little consideration explained all the seeming difficulties of the case. The direction of the wind, or the varying strength of the draught behind the panel, caused our pendulum to vibrate in different ways. Sometimes it struck fully

against the wood, sometimes barely grazed it, or swung clear altogether. On calm nights, of course, it was motionless. The various noises in the house prevented our hearing it in the daytime. At all events the "Ticking Ghost" was laid, and we afterwards slept in peace. M. B.

MEAL-WORMS.

[1196]—As a reader of KNOWLEDGE I have been much interested in your entomological articles, and I venture to ask whether you can include in your series some account of the creatures called "Meal-Worms," or, if your scheme will not admit of this, will you be so kind as to tell me of some book which will give me information on the subject?

I have attempted to breed them, after the recipe of the great German authority on cage-birds, Bechstein (I keep a few birds), but with very indifferent success (I fancy I drowned them with too much beer). But I have been greatly interested in trying to follow the stages of their development. They seem to be continually shedding their skins, and at length acquire a most quaint appearance—like tiny ghosts they look.

Now I want to know whether these queer-looking, corpse-like things are the pupæ, and if so, how long do they remain in this state? And again, how is it that the black, horny beetles, which I take to be the perfect insects, vary so much in size? I thought that perfectly-developed insects never grow.

If you will favour me with some information on these points, either in the shape of an article in KNOWLEDGE or a paragraph in "Answers to Correspondents" (which I am sure the Editor would grant) I shall be very grateful; and I may be allowed to add that in your hands the subject would prove generally interesting.

S. A. Butler, Esq. F. M. DUPLOCK.

CHEESE.

[1197]—Some little time ago Mr. Williams published in your paper an article showing how to treat cheese to make it easy of digestion. I procured some cheese and bicarbonate of potash, and treated it according to the directions given; but, instead of getting a nice creamy substance, the cheese formed a toughened gluten-like paste, that nothing with a stomach like a human being could digest, I should fancy. Would you kindly give publicity to my experience, so that Mr. Williams may have a chance of setting us right in the matter? J. P. CARTER.

AMERICAN BACON.

[1198]—I lately received a letter from a young man well known to me, who emigrated some months ago to the north-western part of the United States. After describing his work there, he adds, "Don't you ever eat American pork. Its education is infinitely worse looked after than that of the Indian (pig). Any cow, or other animal that dies of disease, is thrown to them. I have often been told to do this myself. Other feeding they get too bad to mention."

This would appear to show that there may be reason in the scare both in England and in Germany regarding American hams and bacon.

A day or two since, when speaking of this to a friend whose home is in Yorkshire, he said that the labourers in his neighbourhood have found from experience that it is more economical to pay the higher price for English bacon than to buy that which comes from America; the latter, as they express it, "does not stay in the frying-pan."

I am in hopes that Mr. Mattieu Williams, who has written such interesting papers on the "Chemistry of Cookery," may be able to explain whence this difference arises. I have observed that slices of American bacon, when cooked and served at table, seem almost to shrink into nothing. Is this due to the breed of pigs or to some other cause, or can it be that "American" is a trade name for inferior bacon? COSMOPOLITAN.

LETTERS RECEIVED AND SHORT ANSWERS.

R. N. It is easy to devise many geometrical constructions for such an approximation as that. They would have no scientific value, nor any bearing on the problem of the quadrature of the circle. —D. S. MILLAR. Valentia, not Valencia. Authority, every respectable atlas.—W. H. SHARP. The notice to which you take exception was not written by me; and my short article on the Mystery of Gravity was written two years since, long before your book appeared. If your views are just they will prevail. I have not read your book myself, but I have the fullest confidence in the reviewer's judgment.—ULTIMA THULE. Cannot promise to look out answers to all such questions, as KNOWLEDGE does not claim to be a question-answering paper. But the following information chances to be handy by:—Brorsen's Comet,—Least distance from sun 0·62, greatest 5·66, eccentricity 0·8098, inclination 29° 23′, longitude of node 101° 20′; distance from node to perihelion 14° 55′, periodic time 5·561. Next perihelion passage October in the present year. For D'Arrest's the corresponding elements are respectively 1·17, 5·72; 0·6278; 15° 43′; 146° 9′; 173° 0′; 6·39; 1880, March. —ROUND SHOULDER. Club exercises such as recommended in "How to Get Strong" for development of pectoral muscles would be better than any artificial bracing.—F. W. HALFPENNY. "Honesty is the best principle" may be grammatically correct; but it does not express the same idea as "Honesty is the best policy." The word "policy" means "the art of government," as you say; but it has another meaning, to wit, plan or system of managing. The proverb means that it is good for his own interest that a man should be honest. "Honesty is the best principle" would be a far better saying, meaning that whether good for a man's interest or not, honesty is the right thing.—H. But has any man ever for a moment imagined that men can conceive either infinite space or infinite time? Until some one has claimed to do this, or even imagined that it might be done, your letter seems wanting in point. We cannot reply about the wrong views till we know what are the views attacked and the reasoning urged against them. Attacks on straw-stuffed giants would hardly suit these columns.—A PERMANENT SUBSCRIBER. The question raised in your letter respecting Heis's Celestial Atlas would involve too much technical writing to be suitable for our Correspondence columns.

Our Parador Column.

THE FLAT EARTH AND ITS FLATTENER.

I WAS somewhat surprised to read your remarks in this week's KNOWLEDGE, not so much at their tenour, which I cordially endorse, as at the circumstances that called them forth. I was, indeed, warned by Mr. Hampden in a very characteristic letter, that I should forthwith publish an apology, or, says he, "*if* Parallax *should* consult his solicitor," the latter is sure to advise him to institute criminal proceedings against me and you, sir, for my second article in your journal. This was all I heard from the Zetetic camp about proceedings. I presume, if such had really been instituted, I should have heard something about them by this time. But up to the present I have had no intimation whatever to lead me to suppose that Parallax will take such a step. If he is not ill advised, he will know as well as I do that it would be necessary to prove, in the first instance, that my articles were libels. If I had touched him in his private capacity, then the matter would be different. My comments were, however, directed against him in his public capacity, and as such could not be looked at otherwise than as a fair criticism, unless, indeed, they had contained false statements. Parallax will be aware what an action for libel can* lead to, and as an old friend—although [our friendship was] of short duration—I should suggest what you, sir, did in your remarks, viz., that he should make use of the columns of your Journal to offer an emphatic contradiction to my statements. Should be succeed in clearing up the mysteries in which he stands for the present enshrouded I will join with you, sir, and make the fullest apology. He is bound to take now either one or the other step. So far as I am concerned I am willing to stand or fall by my statements. But he should lose no time, as delay will justify you in inserting my further articles.

Thanking you for the corrections, which convey really what I meant to say, H. OSSIPOFF WOLFSON.

April 12, 1884.

[I also would venture to point out to Dr. S. Birley that it will be his own fault *if he libel himself by silence*, when space is here offered him for full contradiction of whatever in Mr. Wolfson's statements he may be in a position to deny. His solicitor, Mr. Howard Rumney, writes to the publishers of KNOWLEDGE that both Mr. Wolfson's letters contain untrue statements. What are they? Is he not Parallax? His solicitor says he is. Is Parallax not the same as Goulden, Rowbotham, and Tryon. But at Trowbridge in

* I leave this word unchanged, as it confirms my interpretation of the same word used for "may" in Mr. Wolfson's second letter. —R. P.

1849, the author of the Zetetic Astronomy, who is Parallax, was Goulden; and in 1864 the same Parallax was Rowbotham. Is Dr. Birley a registered medical practitioner? I have the "Medical Register" before me, and there is but one Birley, Mr. (not Dr.) C. Birley, registered in 1859, in its lists. Parallax is unquestionably the writer of a book promising particular longevity. And all Parallax has done or written he has widely advertised. What is it in Mr. Wolfson's letters he desires to contradict?—R. P.]

Our Mathematical Column.

NOTES ON EUCLID'S FIRST BOOK.

By Richard A. Proctor.

(*Continued from p.* 234.)

NOW if we collect the properties of isosceles triangles involved in the three last-named propositions, we see that they present themselves as in the following propositions:—

Prop. VIII.—*The bisector of the vertical angle of an isosceles triangle bisects the base also.* This is established in the proof of Euc. I., 10.

Prop. IX.—*The bisector of the vertical angle of an isosceles triangle is at right angles to the base.* This is established in the proofs of Euc. I. 10 and 11.

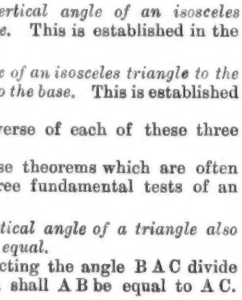
Prop. X.—*The line joining the vertex of an isosceles triangle to the bisection of the base is at right angles to the base.* This is established in the proofs of Euc. I. 11 or 12.

It is obvious that the direct converse of each of these three propositions is also true.

But there are three indirect converse theorems which are often useful. They may be called the three fundamental tests of an isosceles triangle:—

Prop. XI.—*If the bisector of the vertical angle of a triangle also bisects the base, the other two sides are equal.*

In the triangle B A C, let A D bisecting the angle B A C divide B C into two equal parts in D. Then shall A B be equal to A C.



In the triangles B A D, C A D, we have the sides B D, D A, equal to the sides C D, D A each to each, and the angles B A D, C A D, which are opposite the equal sides B D, C D are likewise equal. Hence by Props. I. and II. the triangles are either equal in all respects, or else the angles B and C together make up two right angles. But the angles B and C, being two angles of a triangle are together always less than two right angles. Hence the triangles A B D, A C D are equal in all respects. Therefore A B is equal to A C.

Prop. XII. *If the line drawn from the vertex of a triangle to the bisection of the base is perpendicular to the base, the other two sides are equal.*

If (same figure) B D is equal to D C, and A D perpendicular to B C, the triangles A B D, A C D are equal in all respects by Euc. I. 4.

Prop. XIII. *If the bisector of the vertical angle of a triangle is perpendicular to the base, the sides are equal.*

If (same figure) the angle B A D is equal to the angle D A C, and A D also at right angles to B C, the triangles A B D, A C D are equal in all respects by Euc. I. 26.

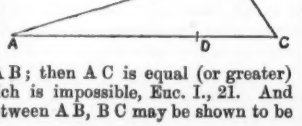
In Props. 13-15, Euclid exhibits properties of straight lines which are often useful in determining whether three or more points lie in a straight line, and also whether three or more lines pass through one point.

Props. 16-21 are of continual use in solving problems, as we shall see further on.

The following proposition is often useful:—

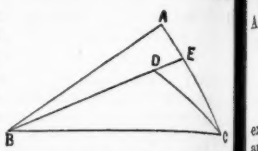
Prop. XIV.—*The difference between any two sides of a triangle is less than the third side.*

From A C, a side of the triangle A B C cut off D C equal to B C; then the remainder A D is less than A B. For if A D be equal (or greater) than A B, add D C to A D, and add B C, which is equal to D C, to A B; then A C is equal (or greater) than A B and B C together which is impossible, Euc. I., 21. And in like manner the difference between A B, B C may be shown to be less than A C; and the difference between A C and A B less than B C.



It is well to note that in place of the general theorem which forms the latter part of Prop. 21, we may substitute the following useful proposition:—

Prop. XV.—*If from the extremities B C of the base B C of a triangle B A C, the lines B D, C D be drawn to a point D within the triangle, then the angle B D C exceeds the angle B A C by the sum of the angles A B D and A C D.*

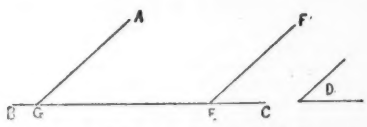


For the angle B D C is equal to the two angles D C E, D E C, Euc. I., 32; that is, to the three angles D C E, B A E, and A B E, Euc. I., 32.

The following propositioni s as often applicable as Prop. 23:—

Prop. XVI.—*Prob. From a given point without a given line to draw a line which shall make with the given line an angle equal to a given rectilinear angle.*

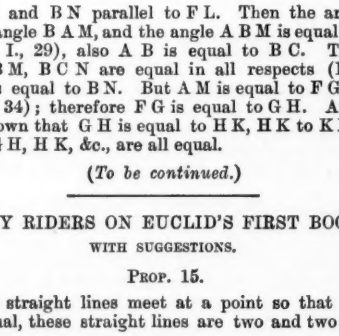
Let A be the given point, B C the given line, and D the given angle.



From any point E in B C draw E F so that the angle F E C may be equal to the angle D (Euc. I., 23). Through A draw A G parallel to F E. Then the angle A G E is equal to the angle F E C (Euc. I., 22) that is to the angle D.

Props. 27-31 exhibit the properties of parallels. To these the following very useful property may be added.

Prop. XVII.—*If there be any number of parallel lines A F, B G, C H, &c., and if any straight line A E meeting the parallels in the points A, B, C, D, &c., be divided into equal parts A B, B C, C D, &c., then any other straight line F L, meeting the parallels in the points F, G, H, K, &c., will be divided into equal parts F G, G H, H K, &c.*



Draw A M and B N parallel to F L. Then the angle C B N is equal to the angle B A M, and the angle A B M is equal to the angle B C N (Euc. I., 29), also A B is equal to B C. Therefore the triangles A B M, B C N are equal in all respects (Euc. I., 26). Hence A M is equal to B N. But A M is equal to F G, and B N to G H (Euc. I., 34); therefore F G is equal to G H. And similarly it may be shown that G H is equal to H K, H K to K L, and so on. Hence F G, G H, H K, &c., are all equal.

(*To be continued.*)

EASY RIDERS ON EUCLID'S FIRST BOOK.

WITH SUGGESTIONS.

Prop. 15.

39. If four straight lines meet at a point so that the vertical angles are equal, these straight lines are two and two in the same straight line.

40. If A B C, D B E are two straight lines intersecting in B, and A B is equal to B D, B E to B C; show that the quadrilateral A D C E is made up of four triangles whereof two are isosceles and the other two equal in all respects.

41. If with the same construction A B is equal to B C and D B to B E, the quadrilateral A D C E is made up of four triangles of which each opposite pair are equal in all respects.

Prop. 16.

42. In the triangle A B C, A D is drawn bisecting the angle B A C, and meeting B C in D, show that the angle B D A is greater than the angle B A D.

43. Through D a point in the base B C of an isosceles triangle A B C, a line E D F is drawn meeting A B in E and A C produced in F; show that the angle A E F is greater than the angle A F E.

44. In the figure of Prop. 17, show that the angles A B C and A C B are less than two right angles, without producing B C.

Join A to a point in B C, and apply Prop. 16 *twice.*

PROP. 17.

45. If two sides of a triangle are produced, show that the two exterior angles thus formed are together greater than two right angles.

46. Show that any three angles of a quadrilateral are together less than four right angles.

PROP. 18.

47. Each of the diagonals of a quadrilateral figure exceeds the greatest side, show that the sum of two opposite angles exceeds half the sum of the remaining angles.

48. A B C D is a quadrilateral of which A D is the longest side and B C the shortest; show that the angle A B C is greater than the angle A D C, and the angle B C D greater than the angle B A D.

PROP. 19.

49. In the figure of Euc. I., 5, show that F C is greater than B C.

50. Either diagonal of a rectangle exceeds the greatest side.

51. Either diagonal of a rectangle exceeds any line which has one extremity at an angle, and the other on a side of the rectangle.

52. The perpendicular is the shortest straight line that can be drawn from a given point to a given straight line; and of others that which is nearer to the perpendicular is less than the more remote; and only two equal straight lines can be drawn from the given point to the given straight line, one on each side of the perpendicular.

53. P and Q are points on the same side of the line A B; P C is drawn perpendicular to A B, and produced to D so that C D is equal to P C; show that Q D is greater than P Q.

Let Q D cut A B in E, and join E P: with this construction the proof is obvious.

PROP. 20.

54. The difference of any two sides of a triangle is less than the third side.

55. A point P is taken within the triangle A B C; show that the sum of the distances P A, P B, and P C is greater than half the sum of the sides of the triangle.

56. With the same construction as in Ex. 53, a point F is taken in A B; show that the sum of the lines P F and Q F is greater than Q D.

57. A B C is a triangle having the angle B obtuse. A point D is taken in B C, and in A D, D E is taken equal to A B, and E A is bisected in F. Show that C F and D F are together greater than C A, A B.

58. The diagonals of a quadrilateral are together less than the sum of any four straight lines that can be drawn to the four angles of the quadrilateral from any point whatever except the intersection of the diagonals of the quadrilateral.

59. The sides of a quadrilateral are together greater than the two diagonals together.

60. In the triangle A B C the line B D is drawn bisecting the vertical angle A B C. If any point E is taken in B D, show that the difference of the sides A B, B C exceeds the difference of the lines A E, E C.

From A B the greater of the sides A B, B C (suppose) cut off B F equal to B C the less; show that E F is equal to E C; and apply Ex. 54.

61. With the same construction, show that if the point E lies in B D produced either way, the difference of A B, B C exceeds the difference of A E, E C.

62. A straight line A D is divided into two unequal parts in the point C, and a straight line C D is drawn at right angles to A D. Show that the difference of the lines A D, B D is less than the difference of the lines A C, B C.

PROP. 21.

63. A point P is taken within the triangle A B C; show that the sum of the lines P A, P B, and P C is less than the sum of the sides of the triangle.

64. A B C D is a quadrilateral whose diagonals intersect in E, and a point F is taken within the triangle A B E. Show that the sum of the diagonals A C, B D together with twice the side A B exceeds the sum of the four lines A F, B F, C F, and D F.

(To be continued.)

Our Whist Column.

A WHIST-PLAYER'S WAIL.

BY THE AUTHOR OF "BUMBLEPUPPY."

(Continued from p. 256.)

LASTLY, with regard to the authority.

Whist-players are law-abiding to a degree, and sufferance is the badge of all their tribe; but still they would like to know how the authority obtained what the imperfect Member for Northampton is so fond of calling his mandate; whether by divine or hereditary right, by competitive examination, by election, by appointment from the Crown, or whether he sits upon us by "the good old rule, the simple plan" *of force majeure*, as the Old Man of the Sea sat upon Sindbad.

Bartholomew Binns, an official with the highest credentials, after being selected from numerous candidates, and receiving a mandate from the sheriffs of London and Middlesex, has his decisions reviewed by twelve good men and true, and reporters are present who publish them through the length and breadth of the land? How is our executioner appointed? Who reviews his decisions? How are they promulgated? Not that it matters to me, personally. When my fatal Monday comes round, and *sus. per coll.* is written under my name in the family archives, I do not imagine it will trouble me much whether the operator was born great, has achieved greatness, or has had greatness thrust upon him. I do not object to the instrument, I object to the system; but many Whist-players are more fastidious, and protest strenuously against being treated worse than other criminals. They hold that the position of a functionary who takes upon himself to decide important questions of law, and to upset old-established precedents, and manufacture new ones on his mere *ipse dixit*, should be very clearly defined, and that if one man is to unite in his own proper person the attributes of prophet, priest, and king—three single gentlemen rolled into one—he should be duly anointed, consecrated, and crowned, *ad hoc*.

For questions involving common courtesy, for insoluble verbal quibbles, for ethical questions of this type, "Ought A to sit quietly at the table while his partner B picks Z's pocket? and if he ought, is it right for him to share the plunder?" and for the host of minor cases which constantly arise, and for which no law could possibly provide, no better management than the present could be devised. As long as maniacs exist in the land, klepto- dipso- homicidal or whist—offences must come, and in disposing of them—when a cadi is the only effective treatment that can be openly suggested—the editor of the *Field* is *facile princeps*,

> In faith, he is a worthy gentleman,
> Exceedingly well read.

Only if he is to be the *de facto* authority in *all* cases, why not give him the three sanctions just mentioned, and make him the authority *de jure?* Then—as the *Field* is not a Whist gazette, and can scarcely be expected to devote its columns to advertising gratuitously every legislative change, and any space it has to spare is used rather for elaborating the ceremonial than for settling the laws of the club—in token of our esteem, let us club together (I shall be most happy to contribute my mite) and present him with a piece of chalk, a duster, and a black board, to be set up in some easily-accessible spot—say, the middle of Pall-mall or St. James's-street. Make it the official notice-board! When new decisions are created let them be legibly inscribed upon it, *coram populo!* When well-known decisions are abrogated let them be carefully rubbed out at once. Since the Bastille was destroyed and *lettres de cachet* with it, there has been no authority without a notice-board; the Salvation Army has its "war cry," and the Pope himself, when he propounds a new dogma, propounds it *ex cathedrâ*.

That is one remedy. Though it is not perfect it has two advantages—it is inexpensive and if in future any of us should still remain in ignorance, we should be in ignorance by our own fault, and not by misfortune; and at any rate it is a more simple and less tortuous plan than upsetting well-known decisions in an unofficial newspaper, while new editions of our two standard Whist-books are subsequently brought out without one word of comment or warning.

The alternative remedy—by no means novel, it has been suggested, *usque ad nauseam*, and I only bring it forward again because at present confusion is worse confounded than it has ever been in my recollection—is for the leading clubs to appoint a small committee of representative Whist-players, with power to revise any decisions they may see fit; and when they have revised them either to append them to the laws of Whist, or to place each decision as a rider under its own particular law, and every such decision should be final.

Questions of strict law should never have been submitted to an

arbitrator at all; they should have been cleared up long ago by the legislators themselves; though important, they are not very numerous, and as they have been well threshed out, and all their difficulties are known, the entire matter might be completed in a few hours. • Why should London wait? PEMBRIDGE.

Our Chess Column.

By MEPHISTO.

SOLUTION OF PROBLEM No. 115, BY E. N. FRANKENSTEIN, p. 94.

1. R to R5	if K to K6	2. R to K2, mate
	if K to B4	2. B to Q3, mate
	if K to Q4	2. Kt to B6, mate
	if P to Q4	2. Q to K5, mate
	if QKt moves	2. B to Q3, mate
	if KKt moves	2. Q × B, mate
	if B moves	2. Q to B4, mate

Correct solutions received (additional).—G. A. Porritt, H. P. F., W. Hanrahan, Senex, J. G. Barber, J. Wahltuch, Lawrence Small. Incorrect.—G. T. M. E., W. B. Hammond, Field House, W. P. H., A. T. J., Fred Lobnitz, W. C., R. H. Punshon.

SELECTED PROBLEMS.

I.

By S. LOYD.

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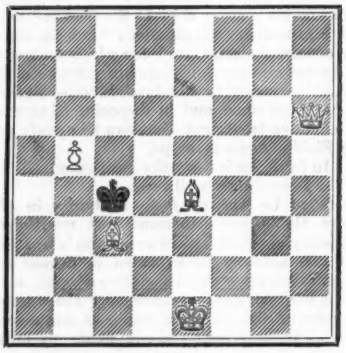

WHITE.

White to mate in two moves.

II.

By S. LOYD.

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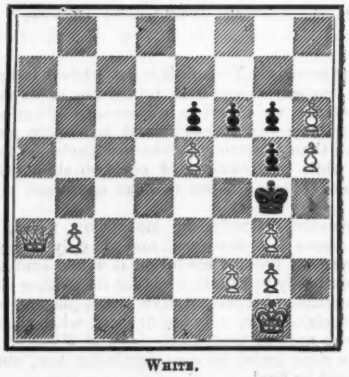


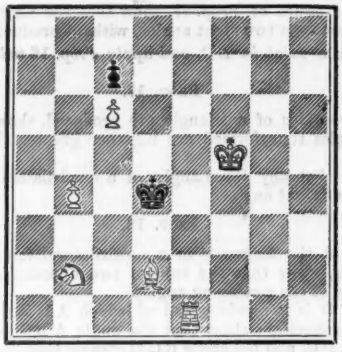
WHITE.

White to mate in three moves.

PROBLEM No. 116.

By H. W. SHEPPARD.

BLACK.



WHITE.

White to play and mate in four moves.

(Both neat and subtle.—CHESS ED.)

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess Editor.*

J. OSBORNE.—The position you refer to is as follows:—White K on QR6, R on Q7. Black K on QKt sq. P on QB7. White draws by R to Kt7 (ch), K to Bsq (best). R to Kt5, P to B8 (Q). R to B5 (ch.) and if Q × R White is stalemate.

WATTER.—We understand that a book on the openings will shortly be published. In reply to 1. P to K, P to K4, 2. Q to R5; besides Kt to QB3, you may also play Kt to KB3. A speedy development is obtained by the sacrifice of the P.

CONTENTS OF No. 128.

	PAGE		PAGE
Coincidences and Superstitions. By R. A. Proctor	237	Ichneumon Flies. (*Illus.*) By E. A. Butler, B.A., B.Sc.	244
The Chemistry of Cookery. XXXII. By W. M. Williams	238	Reviews	245
Pleasant Hours with the Microscope. By H. J. Slack, F.G.S., F.R.M.S.	240	Map of Constellations. (*Illus.*) By R. A. Proctor	247
The Morality of Happiness. By Thomas Foster	240	Editorial Gossip	247
How to Get Strong	242	The Face of the Sky. By F.R.A.S.	248
The Permanence of the Domestic Instinct in the Cat	243	Correspondence	249
Electro-plating. III. By W. Slingo.	243	Our Paradox Column	253
		Our Mathematical Column	253
		Our Chess Column	254
		Our Whist Column	256

SPECIAL NOTICES.

Volume IV., comprising the numbers published from July to December, 1883, is now ready, price 7s. 6d.; including parcels postage, 8s.

The Title-Page and Index to Vol. IV. also ready, price 2d.; post-free, 2½d.

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Remittances should in every case accompany parcels for binding.

The price of the Monthly Parts will in future be 1s. for those containing four numbers, and 1s. 3d. for those containing five.

Part XXIX. (March, 1884), now ready, price 1s., post-free, 1s. 3d.

MR. R. A. PROCTOR'S COURSE OF LECTURES.

1. LIFE OF WORLDS.
2. THE SUN.
3. THE MOON.
4. THE PLANETS.
5. COMETS.
6. THE STAR DEPTHS.

See Advt. Pages for full Syllabus.

The following arrangements are complete: the numbers in brackets referring to above list.

BIRMINGHAM (Town Hall), April 18, 23, 25, 28; May 2 (1, 2, 3, 5, 6).

LEAMINGTON (Royal Music Hall), Four Afternoons, April 17, 19, 24, 26 (1, 3, 5, 6); Two Evenings, April 17, 24 (2, 4).

St. HELEN'S (Lanc.), April 22 (2).

COVENTRY, April 30, May 1 (1, 2).

MALVERN, May 3, 17 (Afternoon) (2, 3).

LLANELLY, May 5 (1).

SWANSEA, May 6, 7 (1, 2).

WORCESTER, May 8, 9, 15, 16.

BANBURY, May 14 (Afternoon and Evening).

OXFORD, May 12, 13, 19, 20.

CAMBRIDGE, May 21, 22, 23.

NOTE.—*All communications respecting Lectures should be addressed to Mr. John Stuart, Royal Concert Hall, St. Leonards.*